ANNALS OF PHILOSOPHY;

OR, MAGAZINE OF

CHEMISTRY, MINERALOGY, MECHANICS,

NATURAL HISTORY,

AGRICULTURE, AND THE ARTS.

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VOL. VII.

JANUARY TO JUNE, 1816.

London:

Printed by C. Baldwin, New Bridge-street;

FOR BALDWIN, CRADOCK, AND JOY,

47, PATERNOSTER-ROW.

SOLD ALSO BY

W. BLACKWOOD, EDINBURGH; AND J. CUMMING, DUBLIN.

1816.

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ANNALS

OF

PHILOSOPHY.

JANUARY, 1816.

Article I.

Account of the Improvements in Physical Science during the Year 1815. By Thomas Thomson, M.D. F.R.S.

THE following account will be much less complete than I could have wished; but I was prevented by illness from commencing it till within three weeks of the period at which it was to be sent to the press. I was unable, in consequence, to peruse the vast number of papers and books which it was requisite to read (above 200 in number) with that attention which would have been necessary in order to give a complete account of their contents. I have, however, endeavoured to do as much justice to the subject as the shortness of the time would allow.

I. MATHEMATICS.

The observations of Professor Christison on the nature of fluxions (*Annals of Philosophy*, vol. v. p. 328, vol. vi. p. 178, 420,) will be read with interest by all those who wish to understand the metaphysics of this branch of mathematics, and to be satisfied of the accuracy of the calculus.

Only one mathematical paper has been published in the Philosophical Transactions for 1815, namely, an Essay towards the Calculus of Functions, by C. Babbage, Esq. The term *function* has long been used in analysis for the purpose of denoting the result of every operation that can be performed on quantity. The author of this curious and important paper first explains the notation which he makes use of, and the various orders of functions which may occur. He then solves 20 different problems, and shows their application to the solution of various interesting questions.

II. ASTRONOMY.

Three astronomical papers have been published in the Philosophical Transactions for 1815.

1. A memoir by Dr. Herschel on the Satellites of the Georgian Planet. It contains an immense collection of observations, continued from 1787 to 1810. The existence of two satellites has been established completely; the first of which performs a synodical revolution about the planet in $8^d\ 16^h\ 56'\ 5{\cdot}2''$; the second, in $13^d\ 11^h\ 8'\ 59''$. He has rendered it probable that there exists a satellite nearer the planet than either of these two, and that there are likewise several exterior satellites. But the extreme remoteness of this planet renders the determination of these points exceedingly difficult.

2. A Memoir on the Dispersive Power of the Atmosphere, and its Effect on Astronomical Observations, by Mr. Stephen Lee. The author observes that stars of different colours must be differently refracted, and that the apparent altitude of the sun must vary according to the colour of the dark glass through which he is viewed. That the fixed stars differ from each other in respect to the composition of their light is evident to the naked eye; but this difference becomes still more perceptible when they are viewed through a prism properly adapted to the eye-piece of a reflecting telescope. The planets also differ much from each other in this respect. These considerations inducing Mr. Lee to suspect that the dispersive power of the atmosphere must be sufficient in many cases to produce considerable effect on astronomical observations, he made a set of observations on the diameter of the planet Mars while in opposition in 1813. From a great number of observations, he found that the deviation of the extreme rays of light was between $\frac{1}{60}$ and $\frac{1}{70}$ part of the total refraction. Mr. Lee conceives that the disagreement between the latitude of a place deduced from observations of circumpolar stars, and from observations of the sun, may be traced to the use of dark glasses. To a similar cause he ascribes some other discordances in astronomical observations.

3. Determination of the North Polar Distances, and proper Motions of 30 fixed Stars, by the Astronomer Royal. The table of the north polar distances of these stars laid before the Royal Society in 1813 was so accurate, that Mr. Pond found no occasion, from his subsequent observations, to make a greater alteration in any of them than $\frac{1}{10}$ of a second. By comparing his own catalogue with that of Dr. Bradley in 1756, he has ascertained the proper motions of these stars during a period of 58 years. The annual proper motion of the Pole Star is $-\ 0{\cdot}057''$; that of β Ursæ Minoris, $+\ 0{\cdot}1''$.

III. ACOUSTICS.

Some objections to Dalton's theory of gases have been started in Germany. The most important of these is, that if the gases are not elastic to each other, every sound ought to be repeated four times,

as we live in an atmosphere composed of four elastic fluids; or supposing the effect of the carbonic acid gas and the vapour of water to be insensible, still every sound ought to be repeated at least twice by the azotic and oxygen atmospheres. As this never happens, it is concluded that these two gases are elastic to each other. This objection having been considered long ago, it is needless to resume the subject here.

The distance at which sounds may be heard is much greater than is generally imagined. Dr. Derham informs us, on the authority of S. Averrani, that at the siege of Messina the report of the guns was heard at Augusta and Syracuse, almost 100 Italian miles distant; and he states upon his own authority that in the naval engagement between the English and Dutch which took place in 1672 the report of their guns was heard upwards of 200 miles off, as far as Shrewsbury and Wales. (Phil. Trans. vol. xxvi. p. 2. 1708.) Humboldt mentions the reports of volcanoes in South America heard at the distance of 300 miles; and Mr. Monro, a British planter in Demerara, informed a friend of mine, on whose statement I can rely, that the loud explosions which took place from the volcano in St. Vincent's were heard distinctly at Demerara. Now this is a distance which must considerably exceed 300 miles.

IV. OPTICS.

The various experiments that have been made by different philosophers to determine the relative quantities of light which proceed from luminous bodies are known, I presume, to most of my readers. The curious results obtained by Bouguer and Lambert, the photometer of Count Rumford, and of Professor Leslie, deserve to be studied and understood by all who are interested in such pursuits. Lampadius has lately proposed a new photometer, and he informs us that he has succeeded in making his instruments agree with each other as accurately as different thermometers do. His photometer consists essentially in a tube a foot long, through which he looks at the luminous object. At the extremity of the tube furthest from the eye he places thin shavings of horn till he can no longer distinguish the luminous object. At first he reckoned the degree of light given out by the luminous body by the number of shavings of horn necessary to intercept it; but as instruments constructed on such a plan could not be comparable with each other, he fell upon the following method to graduate his photometer. He burns phosphorus in oxygen gas, and ascertains the thickness of horn shavings necessary to intercept the light; and he contrives, by means of a screw and a ring, to pack these shavings always so that they shall occupy nearly the same space. This space he divides into 100 degrees. The instrument, thus graduated, serves to measure the light emitted by other luminous bodies. The defects of such an instrument must be apparent to every person. The difference in the transparency and thickness of the horn shavings, and the difficulty of packing them so that they shall always occupy the same space, must render

the instrument difficult of execution. Nor is it impossible that the polarization or non-polarization of the luminous rays according to the nature of the surface from which they proceed may have considerable influence upon the quantity of light capable of reaching the eye through the horn, upon which the value of the instrument as a photometer totally depends. Lampadius has given various tables of the light emitted by different bodies as measured by his instrument. I shall transcribe one, by way of specimen. It exhibits the light emitted by the sky in a clear morning on the 16th of February at Freyberg. The photometer was directed to the south-east at the height of 45° above the horizon.

		Photometer.			Photometer.
4h	0′	8°	6h	40′	50°
5	20	8	6	50	54
5	30	16	7	0	58
5	40	20		The sun rose.	
5	50	26	7	10	60
6	0	30	7	20	62
6	10	34	7	30	63
6	20	40	7	40	63
6	30	46	7	50	63

From this table we see that the twilight began an hour and a half before sun-rise.

But the greatest additions that have been made to the science of optics during the course of the year consist in the investigations respecting the properties of different bodies as far as the polarization of light is concerned. The principal experimenters on this subject have been Dr. Brewster and M. Biot. Dr. Brewster has been most indefatigable, and has published, during the course of 1815, no less than six different memoirs on the subject; five in the Philosophical Transactions of London, and one in the Philosophical Transactions of Edinburgh. I shall first notice such of the memoirs of Biot as have come to my knowledge, and then I shall give an account of Dr. Brewster's discoveries.

1. M. Biot discovered that the tourmaline, when very thin, refracts doubly, like calcareous spar; but when in thick plates, it refracts only singly. From this it is evident that in this mineral there exist two distinct causes of polarization; one belonging to the crystalline molecules of the tourmaline, the other depending on the plates of which the crystal is composed. The first acts sensibly only when the mineral is very thin; the second, when it has a certain degree of thickness.

M. Biot ascertained likewise that, when the agate is very thin, it transmits light in every direction, and possesses the properties of a doubly refracting body. The laws observed by Dr. Brewster respecting the agate hold only when it possesses a certain degree of thickness.

2. After Malus had discovered the polarization of light, when

reflected from the surface of diaphanous bodies, he examined the metals, and found that polarization was not produced by reflection from them, at least in the same manner as from diaphanous bodies. But Dr. Brewster afterwards discovered that, when a ray of light already polarized is reflected several times from the surface of plates of silver or gold, it is modified in such a way that, when analyzed by means of a prism of Iceland spar, it divides itself into two differently coloured pencils. Biot, on repeating the experiment, observed that the colours of the pencils were precisely the same with the coloured rings observed by Newton. These observations did not coincide with those of Dr. Brewster; but upon mentioning the subject to M. Arago, that Gentleman stated that he had obtained results similar to those of Dr. Brewster, and furnished Biot with a plate of silver by means of which that philosopher was enabled to observe similar results. Surprised at this difference, he investigated the subject with care, and found that the phenomena depended on the way in which the metallic plate had been polished. There are two ways of polishing metallic plates; by hammering and by friction. When the former mode is followed, the phenomena observed by Biot are obtained; when the latter, the phenomena observed by Dr. Brewster. Biot at last ascertained that a metallic surface polished by friction produces two distinct effects upon light. It gives to a part of the incident light what he calls *moveable polarization*, the same which is produced by a thin crystallized plate. This occasions the series of coloured rings of Newton. It gives also to the white incident light a fixed polarization in the plane of incidence, the same as is produced by a thick crystalline plate. The first of these polarizations is only sensible in particular positions when the metallic plate is polished by friction. Hence the reason why it was not observed by Dr. Brewster; but it is strong when the plate is polished by hammering, and accordingly it was observed by Biot.

3. M. Biot showed long ago that when light traverses certain crystals, the repulsive force which produces the extraordinary polarization acts with more intensity on the violet molecules than on the blue, more on the blue than on the green, and so on, acting with least intensity upon the red ray. It is natural to conclude that the extraordinary refraction acts in the same manner on the molecules of light, since it is intimately connected with polarization. In a memoir published in the Annales de Chimie for June last (vol. xciv. p. 281,) he has shown that this law holds with respect to Iceland crystal, and indeed all crystals in general.

I shall now give a short account of the discoveries on this subject published by Dr. Brewster during the year 1815.

1. He found that the glass tears formed by dropping melted glass into water, and commonly called Prince Rupert's drops, have the property of depolarizing light like crystallized bodies. He observed cleavages in these glass drops, as in crystals. When sufficiently heated, and allowed to cool slowly, they lose the property of depo-

larizing light. Hence it is obvious that heat produces a crystalline texture in glass, and that the sudden cooling preserves that texture.

2. Dr. Brewster ascertained that the following bodies have the property of depolarizing light, and therefore have a texture analogous to that of crystals :—

Gum-arabic.
Cherry-tree gum.
Caoutchouc.
White wax.
Mixture of resin and white wax.
Cells of the bee.
Manna.
Camphor.
Balsam of Tolu.
Withered film at the root of Calla Ethiopica.
Fibres of flax, hemp, and cotton.
Thin white semi-transparent leaf of sea weed.
Adipocire from muscular fibre.
Ditto from the Innocents' burial ground, Paris.
Ditto from biliary calculi.
Benzoic and oxalic acids.
Spermaceti.
Goldbeaters' leaf.
Transparent and common soap.
Human hair.
Bristles of a sow.
Fibres of silk and wool.
Silk-worm gut and sheep gut.
Human cuticle.
Parchment.
Horny excrescence on the human foot.
Transparent film at the joints of the claws of the common crab.
Human nail.
A quill, and the thin film which lines the inside of it.
Cartilaginous breast-bone of a chicken.
Transparent cartilage from the shoulder of a sheep.
Transparent edge of the feathers of a quill.
Down of goose and ostrich feathers.
Flat bones of a cod.
Cylindrical bones of fish.
Ivory.
Whalebone.
Horn.
Mother-of-pearl.
Bladder of a cow.
Human cornea.
Cornea of a cow.
Cornea of a fish.
Glue.
Hard isinglass.
Acetate of lead.
Glass of borax.
Amber.
Gum-animé.
Sulphur.
Ice.
Oil of mace.
Tallow.
Tortoise-shell.
Heated glass.
Rupert's drops.
Semi-transparent extremity of the legs of a crab.
Tubular film from the body of a crab.

Dr. Brewster found that the following substances have no effect in depolarizing light :—

Gold leaf.
Some crystals of diamond.
Common salt.
Fluor spar.
Spinell.
Sal-ammoniac.
Rochelle salts.
Nitrate of lead.
Sclerotic coat of a fish.
Crystalline lens of a fish.
Crystalline lens of a cow.
Capsule of the lens of a fish.

Ambergris melted and cooled.
Film of hydatids.
Film lining the ribs of a lamb.
Film from the stalks of rhubarb.
Film covering the shell *solen ensis*.
Resin of bile melted and cooled.
Jelly from calves feet.
Skin of a fowl.
Scale from the body of a bee.
Hair of a bee.
Wing of a bee.
Wing of a house beetle.
Wing of the May fly.
Wing of the stone fly.
Hair from the *pinna marina*.
Wing of the meloe vesicatorius.
Film covering the stem of leontodon taraxicum.
Film between the coats of an onion.
Film of the leaf of American houseleek.
Leaf of the hydrangea.
Spatha of a lily.
Film of gum-arabic.
Rosin.
Copal.
Thin fragments of gum-animé.
Galbanum.
Gum Juniper.
Canada balsam indurated.
Sphere of sea weed.
Film on the stalk of *fleur de lys*.
Thin slices from a wafer.
Pappus of leontodon taraxicum.
Film lining the shell of an egg.
Skin of a dried grape.
Phosphorus.
Hair from the fur of a seal.
Skin of an infant eleven months old.
Skin of a child two months before birth.
Skin of a herring.
Mastich.
Burgundy pitch.

Dr. Brewster has shown that the modes in which bodies depolarize light may be reduced to seven :—

(1.) When the crystal possesses neutral axes, and forms two images which are capable of being rendered visible, as in calcareous spar, topaz, &c.

(2.) When the crystal possesses neutral axes, and exhibits only a single image, as the *human hair*, and various *transparent films*.

(3.) When the crystal has no neutral axes, but depolarizes light in every position, as in *gum-arabic*, *caoutchouc*, *tortoise-shell*, &c.

(4.) When there is an approach to a neutral axis, as in *goldbeaters' leaf*, &c.

(5.) When the crystal depolarizes, or restores only a part of the polarized image, as in the *film* of *sea weed* and a *film* from the *crab*.

(6.) When the crystal depolarizes luminous sectors of nebulous light, as the *oil of mace*.

(7.) When the crystal restores the vanished image, but allows it to vanish again during the revolution of the calcareous spar.

Our author gives a theory of these different kinds of depolarization, and endeavours to reduce them all to the first kind.

3. Dr. Brewster discovered that when calves-foot jelly or coagulated isinglass is exposed to pressure, it acquires the property of depolarizing light, and loses it again when the pressure is removed. Thus it appears that by pressure these bodies acquire a crystallized texture.

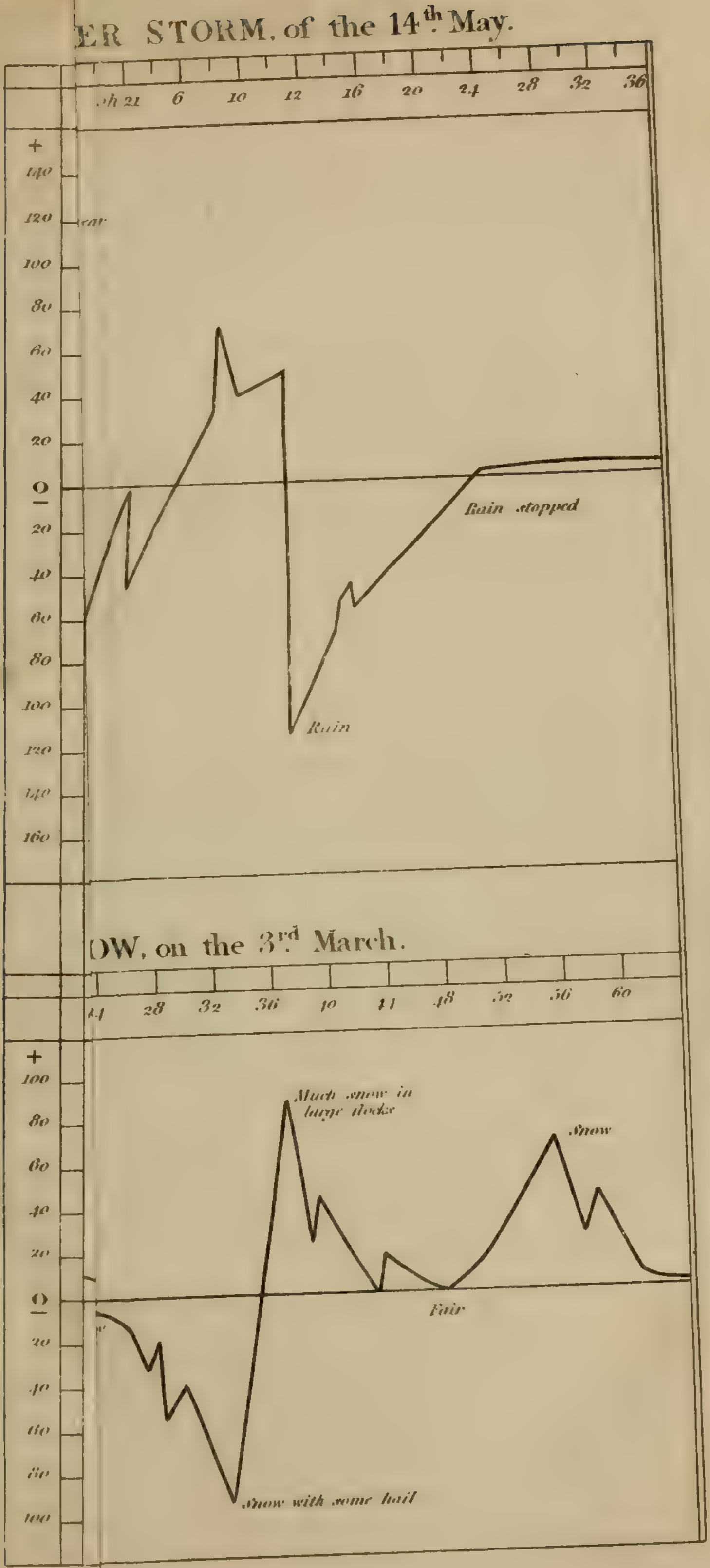
4. Dr. Brewster ascertained by a great number of observations that the *index of refraction is the tangent of polarization.* In the memoir in which he establishes this law he gives a full detail of the laws of the depolarization of light by reflection from the first surfaces of transparent bodies; but I am unable to give an account of this part of his paper without entering into details inconsistent with the nature of this sketch.

5. Some specimens of calcareous spar have the property of multiplying images, and of exhibiting a beautiful set of complementary colours. This was first observed by the late Professor Robison, of Edinburgh, who communicated the fact to Mr. Benjamin Martin. These specimens were examined in succession by Martin, Brougham, and Malus, and they were generally ascribed to fissures in the crystals. Dr. Brewster has shown that fissures are inadequate to produce the effect, that it is owing to a fissure filled up with crystallized calcareous matter, and he has succeeded in imitating these specimens by cementing a thin film of sulphate of lime between two prisms of calcareous spar. The colours are produced by the transmission of polarized light through the crystallized film.

6. When a luminous body is viewed through two parallel plates of equal thickness, placed at the distance of about the tenth of an inch from each other, if one of the glasses be inclined a little, till the reflected image of the luminous body is distinctly separated from the bright image formed by transmitted light, and is received upon the eye placed behind the plates: under these circumstances the reflected image is crossed with about fifteen beautiful parallel fringes. The three central fringes consist of blackish and whitish stripes; and the exterior ones, of brilliant red and green light. These fringes are formed by the joint action of the four reflecting surfaces of the glass, for they are destroyed when the action of any of these surfaces is prevented by coating it with Canada balsam. The direction of these fringes is always parallel to the common section of the four reflecting surfaces which exercise an action upon the incident light. Their breadth is inversely as the inclination of the plates. Their magnitudes are inversely as the thicknesses of the plates which produce them at a given inclination; and in general the magnitude of the fringes is in the compound inverse ratio of the thickness of the plates, and of their angle of inclination. Dr. Brewster conceives that these fringes may be explained by Newton's Theory of Fits of easy Reflection and Transmission of Light.

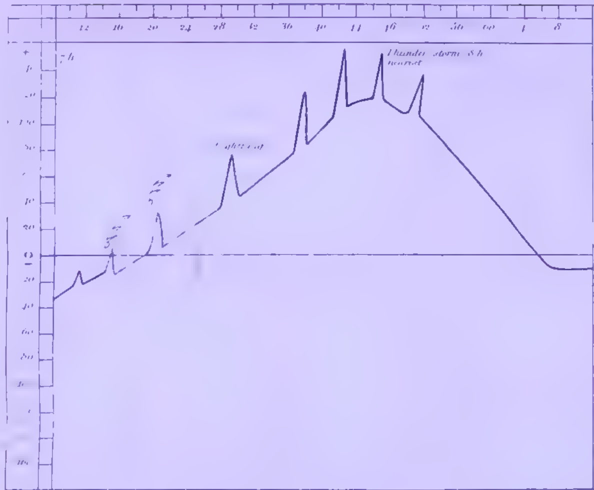
A curious set of observations on the colours exhibited by thin plates of glass placed upon each other, or by a convex lens placed upon a plane glass surface, by Mr. Knox, has been published in the last half volume of the Philosophical Transactions. He merely describes the phenomena which he observed, without attempting to explain them. They consist of certain sets of coloured fringes. They were tangents to the primary coloured rings of Sir Isaac Newton; or, when two sets of primary colours were produced,

Plate XLII.

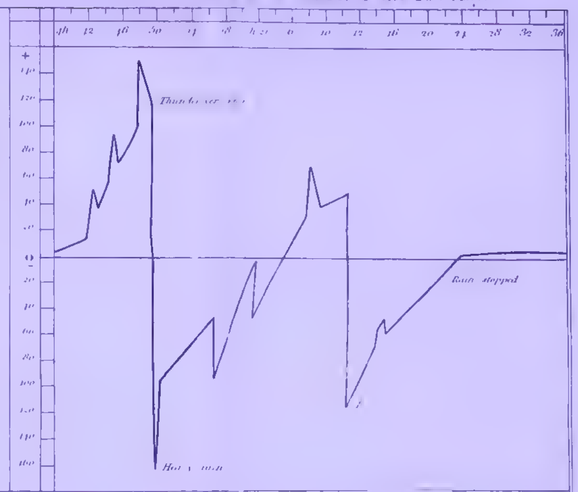


ER STORM, of the 14th May.
Rain
Rain stopped
OW, on the 3rd March.
Much snow in large flocks
Snow
Fair
Snow with some hail

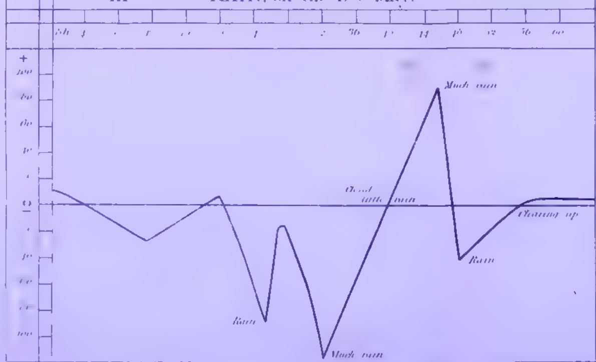
I THUNDER STORM, of the 11th April, 1806.



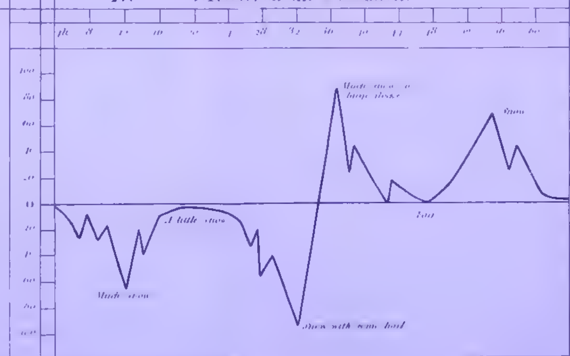
II. THUNDER STORM, of the 14th May.



III RAIN, on the 17th May.



IV. SNOW, on the 3rd March.



they were circles passing through the points in which these primary sets intersected each other. But I shall not attempt a particular description of the phenomena, because they could hardly be rendered intelligible without figures.

V. ELECTRICITY.

1. A set of observations on the electricity of the atmosphere during thunder-storms, rain, and snow, has been published by Dr. Schubler, of Hofwyl. He has given four examples, which are rendered easily intelligible by the graphical representation in Plate XLII. The changes in the state of the electricity take place so suddenly that it requires some dexterity to observe them and note them down. In the plate the horizontal line represents the time from minute to minute; the perpendicular line denotes the degrees of the electrometer; the curve line within the scheme of course represents the position of the electrometer at the time specified in the horizontal line.

The first scheme represents the variation of the electrometer during a thunder storm, which passed by at a distance. Warm weather had preceded April 11, 1806, on which the thunder storm took place; the day was cloudy, with a north-west wind, and a low barometer; the thermometer at two o'clock stood at 57°; the height of the place where the observations were made was 1705 English feet above the level of the sea; the electricity of the atmosphere during the day had been weakly positive; about six in the evening it began to rain, and the electricity of the rain was negative; at seven the rain stopped; but the sky was covered with gloomy clouds, and a thunder storm commenced towards the south-west, with distant thunder and lightning; the electricity of the air continued still negative; but at each flash of lightning its negative state was suddenly diminished, the hand of the electrometer approached 0, but returned almost immediately after the flash nearly to its original position. As the thunder storm approached nearer, the negative state of the atmosphere diminished. At 14 minutes past seven it became suddenly null, and even weakly positive, after a flash of lightning; but almost immediately after resumed its negative state. At 18 minutes after seven it became wholly positive; but the plate will show the changes which the electrometer underwent better than any description. The three remaining examples will be sufficiently understood from the figures. In the second of them the thunder was over head.

2. It is generally known that when a pointed metallic body is attached to the prime conductor of an electrical machine, the electricity does not accumulate in the conductor, but makes its escape from the extremity of the pointed body in a visible stream of light. Professor Hildebrandt has lately made a comparative set of experiments in order to determine which metal, when placed in these circumstances, sends off the greatest visible stream of light. The metals were all made into cones with blunt summits, and they

were put upon the top of a brass rod attached to the upper part of the prime conductor. The following is the order which the metals tried followed, beginning with the one which emitted the greatest quantity of light, and terminating with that metal which gave out the least light:—

Antimony.	Bismuth. }	Iron. }
Gold.	Copper. }	Lead. }
Nickel.	Tin.	Soft steel.
Silver.	Zinc.	Hard steel.
Brass.		

3. It is well known that the phenomena of galvanism have been accounted for by three different hypotheses, and that philosophers are not yet agreed which of these is the true one. According to the first opinion, which is Volta's, the phenomena are purely electrical, and depend upon the different states of electricity in the two metals employed. The chemical phenomena are merely accidental consequences of the electric discharge. According to another opinion, which is that of Berzelius, the phenomena are purely chemical, and the electricity is merely set at liberty in consequence of the chemical actions. A third hypothesis, that of Davy, unites the two preceding ones together, and considers the phenomena as partly electrical and partly chemical.

Professor Pfaff, of Kiel, has published an elaborate examination of these three hypotheses. He endeavours to refute the hypothesis of Berzelius, and to establish that of Volta. I cannot pretend to give an abstract of this memoir here; because it could scarcely be rendered intelligible without a pretty full enumeration of the galvanic phenomena and experiments; an enumeration which would take up much more room than I can possibly spare. But the memoir is well worthy of the attention of all those persons who are interested in electricity or galvanism. I know of no treatise in which a greater quantity of information on the subject is given in less space. (See Schweigger's Journal, x. 179.)

4. De Luc's curious discovery of a dry galvanic pile which continues active for years with little interruption, and likewise his explanation of the voltaic column founded on that discovery, and on his previously published theory of electricity, are known, I presume, to most of my readers, as they were published about five years ago in Nicholson's Journal, and have attracted the general attention of electricians. Zamboni, Professor of Natural Philosophy in the Lyceum at Verona, has made an alteration in the construction of De Luc's pile. He presented one of his *electromotors*, as they have been called, to the Royal Society, and they may be seen commonly enough in the mathematical instrument-makers in London. His pile consists of slips of silver paper laid on each other. On the unsilvered side of the paper is put a layer of black oxide of manganese and honey. These papers are piled above each other to the number of 2000. They are then covered externally with a

coating of shell lac, and inclosed in a hollow brass cylinder. Two of these piles are placed at the distance of four or five inches from each other; and between them is suspended a light metallic needle on a pivot, which is attracted alternately to the one pile and the other, so that it constantly moves between them like a pendulum. Attempts have been made to make this electric pendulum the moving power of a clock or watch; and these attempts have to a certain degree succeeded. Dr. Iäger, of Stuttgardt, has made a number of experiments upon this pile, both as modified by De Luc and by Zamboni. But on looking them over in a cursory manner, I did not perceive any additions of much importance to our previous knowledge.

5. Dr. Wollaston's elementary galvanic battery, described in a late number of the *Annals of Philosophy*, constitutes a discovery of considerable importance. It demonstrates the vast quantity of electricity which is disengaged during the chemical action of acids on metals, and thereby serves to throw much additional light upon the still obscure theory of galvanism.

6. I shall add here the result of Mr. Children's experiments with his immense galvanic battery, though they are mostly chemical; because I could not place them under the department of chemistry, without dividing them so much as to destroy their interest.

The battery consisted of 20 pairs (or rather 20 triads) of copper and zinc plates, each six feet long by two feet eight inches broad, and presenting 32 feet of surface. They were connected together by leaden straps. Wooden troughs filled with water containing a mixture of sulphuric and nitric acids, and each water-tight, were prepared for each triad, and the metals were so suspended and counterpoised that they could be elevated and let down at pleasure. There were two copper plates in each cell, and the zinc plate was interposed between them. The order in which metallic wires connecting the two poles of this battery became red-hot was as follows:

Platinum.	Copper. }	Zinc.
Iron.	Gold. }	Silver.

Tin and lead melting before they became red-hot their place could not be determined. Mr. Children conceives that the metals conveying electricity become red-hot inversely as their conducting power. According to this supposition, the conducting power of these metals for electricity is in the following order:—

Silver.	Gold. }	Iron.
Zinc.	Copper. }	Platinum.

The power of this immense battery may be estimated by the following experiments:—

Five feet six inches of platinum wire 0·11 inch in diameter were heated red-hot throughout visible in day-light.

Eight feet six inches of a platinum wire 0·44 inch diameter were heated red.

A bar of platinum $\frac{1}{6}$ of an inch square, and 2·25 inches long, was also heated red, and fused at the end.

A round bar of platinum, 0·276 inch* in diameter, and 2·5 inches in length, was heated bright red throughout.

The chemical effects of this battery were as follows :—

(1.) Box-wood charcoal intensely ignited in chlorine produced no effect; nor was any effect produced in azotic gas.

(2.) Oxide of tungsten fused, and was partly reduced. The metal greyish white, heavy, brilliant, and very brittle.

(3.) Oxide of tantalum. A very small portion fused. The grains have a reddish yellow colour, and are extremely brittle.

(4.) Oxide of uranium. Fused, but not reduced.

(5.) Oxide of titanium. Fused, but not reduced. When intensely heated, it burnt, throwing off brilliant sparks like iron.

(6.) Oxide of cerium. Fused; and when intensely heated, burnt with a large vivid white flame, and was partly volatilized. The fused oxide, when exposed to the air, fell to powder in a few hours.

(7.) Oxide of molybdenum. Easily fused and reduced. The metal is brittle, steel-grey, and is soon covered with a thin coat of purple oxide.

(8.) Compound ore of iridium and osmium, fused into a globule.

(9.) Iridium, fused into an imperfect globule not free from cavities. The metal was white, very brilliant, and its specific gravity was 18·68.

(10.) Ruby and sapphire were not melted.

(11.) Blue spinell ran into a slag.

(12.) Gadolinite fused into a globule.

(13.) Magnesia was agglutinated.

(14.) Zircon from Norway was imperfectly fused.

(15.) Quartz, silex, and plumbago, were not affected.

(16.) Iron containing diamond was converted into steel, and the diamond disappeared.

VI. MAGNETISM.

1. The magnetometer of Lampadius, described in the *Annals of Philosophy* (vol. iv. p. 434) may be of some use; and might perhaps be improved so as to renderi t a tolerably correct instrument.

2. It is well known that a magnetic needle, if it be poised exactly on a pivot before it receives the magnetic touch, will not retain the horizontal position after it has become a magnet. One end will incline more towards the earth than the other end. This is called the *dip* of the needle. It has been observed, that the end of the needle which is turned to the nearest pole is the one that dips, and the dip increases as we approach the pole, and diminishes as we approach the equator. The dip changes much more slowly

* There must be a mistake in this number. It represents the bar as smaller than the wire, of which eight feet six inches were heated red-hot.

than the declination. Hence it would be of considerable importance to the theory of magnetism to be in possession of a set of good observations of the dip in different latitudes, and might lead to important deductions respecting the position and depth of the magnetic poles of the earth. On that account the following observations by Humboldt, giving the magnetic dip in different parts of the North Atlantic, for the year 1799, are of considerable value.

N. Latitude.	W. Longitude from Greenwich.	Magnetical dip.	Number of oscillations in ten minutes.	
38° 52	18° 42	68·18	242	Good observation.
37 26	18 52	67·81	242	Almost perfect calm.
34 30	19 15	65·70	234	Perfect calm.
31 46	19 24	64·71	237	Doubtful, especially the intensity.
28 28	20 53	62·41	238	Good.
24 53	23 18	60·84	239	Very good.
21 29	28 2	58·18	237	Good.
19 54	31 15	57·27	236	Good.
14 15	50 23	50·67	239	Good.
13 20	55 35	45·60	234	Dip good, intensity doubtful.
11 1	57 11	42·34	237	Good.
10 46	63 14	42·25	229	Good.

3. The variation of the compass, or the alteration which takes place in its declination, or in the point towards which it is directed in different longitudes, was first observed by Columbus. That the declination varies in the same place was first discovered in England, though the name of the discoverer is not accurately known. Wallis ascribes it to Gellibrand, who, according to him, made the discovery in 1645. According to Bond, the discovery was by Mr. John Mair. In the year 1657 there was no variation of the compass in London, or in other words, the needle pointed due north. In 1580 it pointed 11° 15′ east. In 1692 the variation was 6° west. Ever since the year 1657 the declination has been advancing west, and in the year 1814 it was 24° 22′ 22″, according to a mean of the very accurate observations of Colonel Beaufoy, which I consider as superior in precision to any that were ever made before him. At first the declination varied at a considerable rate; thus, during the first 15 years after 1657, the declination had advanced west two degrees and a half, which gives a variation amounting to ten minutes for each year. But of late years this declination has been progressively diminishing, and according to the observations of Colonel Beaufoy, the increase from 1813 to 1814 was only 31″; or 40″, if we confine ourselves to the state of the needle at noon.

Dr. Halley was the first person who endeavoured to form a theory capable of explaining this variation in the declination. He supposed that the earth contained an immense magnet within it, poised upon its axis, and having four poles, two weaker than the

other two. This internal magnet he conceived to move, and this motion occasioned the declination. I think it possible that an internal magnet with four poles might occasion the variation without itself being subjected to motion; for we know that the two poles would act upon each other, and vary each other's intensity. But if such be the cause of the annual variation, a time will come when it will cease; though this time may be far distant.

Dr. Halley conceived the position of the principal north pole to be not far from Baffin's Bay. Mr. Churchman placed it in north latitude 58°, and longitude 134° west from Greenwich; but the following observations by Captain Brown, for which the world is indebted to Colonel Beaufoy, who furnished the compass by means of which they were made, shows that this position is not accurate. (*Annals of Philosophy*, vol. v. p. 368)

Variation.	Latitude.	Longitude.	
79° 42′ W	72° 46′ N	—° —′	By sun and moon.
79 00 W	72 5 N	— —	
78 15 W	71 26 N	— —	
73 44 W	— —	— —	
74 00 W	70 58 N	54 14 W	
73 40 W	70 55 N	— —	
72 00 W	70 5 N	— —	
71 00 W	66 59 N	57 4 W	
70 40 W	65 44 N	59 31 W	
70 00 W	63 40 N	59 22 W	
68 00 W	63 34 N	58 33 W	

Were the position of the principal north magnetical poles of the earth so far south as latitude 58°, the needle in the preceding table would have always pointed to the south of west; whereas its direction is northerly, even in latitude 72° 46′. This might perhaps be accounted for, indeed, by supposing another magnetic north pole to exert some influence on the needle; but Churchman, who only admitted the existence of one magnetic north pole, could not avail himself of such a supposition.

4. But the most important set of magnetical observations are those of Colonel Beaufoy to determine the variation of the needle. The diurnal variation of the needle was discovered by Mr. George Graham; and afterwards a set of experiments was made upon it by Mr. Canton, and by M. Van Swinden, in order to determine its rate at different seasons of the year. The result of these was, that the diurnal variation is greatest in summer and least in winter; and that it increases from eight in the morning till two, when it gradually returns to its original position.

Colonel Beaufoy's observations were made with a much better instrument than had been formerly employed, and they were con-

tinued without interruption for two years and six months. A description and engraving of the instrument employed is given in the *Annals of Philosophy*, vol. ii. p. 96. Three observations were made every day; one about half past eight in the morning, another at noon, and the third in the evening about seven o'clock. The results obviously deducible from these experiments are the following.

(1.) The variation was least in the morning and greatest at noon. The mean variation at the three periods of observing for two years was as follows:

Morning	24°	14′	39″
Noon	24	21	54
Evening	24	16	4·5

So that at noon the declination was 7′ 15″ greater than at half past eight in the morning; and 5′ 49·5″ greater than at seven in the evening.

(2.) The law laid down by Mr. Canton, that the variation is greatest in summer and least in winter; and that it varies with the temperature, does not hold. The variation was greatest indeed during both years about the month of August; but the next greatest variation to that occurred in the month of March. The noon variation for 1813, placing the months in the order of its intensity, was as follows:

August	24°	23′	32″	April	24°	21′	12″
March.....	24	23	8	February ..	24	20	58
July.......	24	23	4	November ..	24	20	54
October ...	24	22	53	May	24	20	54
September .	24	22	32	December...	24	20	30
June......	24	22	17	January ...	24	19	3

Their order for the year 1814 is as follows:

April	24°	23′	53″	May.......	24°	22′	13″
August	24	23	48	February...	24	21	51
July.......	24	23	44	October ...	24	21	45
March.....	24	23	40	November..	24	20	37
September..	24	23	17	December..	24	20	36
June......	24	22	48	January ...	24	20	12

January always gives the least variation, and it may be considered as the coldest month of the year; but the other months present anomalies which cannot well be ascribed to the temperature. Thus March, though usually a very cold month, exhibits a very high variation; while February and May very nearly agree in the quantity of variation.

(3.) The variation between two contiguous days often varies four or five minutes, and the needles sometimes vibrate 7′, or even 14′, without any apparent cause.

(4.) A south west wind seems to increase the variation and the unsteadiness of the needles.

I am inclined to ascribe the diurnal variation to changes pro-

duced in the needles themselves; their magnetic virtue appears to be increased or diminished in consequence of various states of the atmosphere, with which we are but imperfectly acquainted. Temperature is obviously one of the agents in these changes, though not the only one. The dryness and moisture of the atmosphere seems to be another. It would seem as if the magnetic intensity of the needle were increased by a moist atmosphere. It is not unlikely that the electrical state of the atmosphere may also have its influence. The repetition of these experiments, while the state of the hygrometer and electrometer at the same time with the observation of the variation is noted down, would probably throw much new light on this obscure subject.

That the diurnal variation is owing to changes in the needle itself I conclude from a fact observed by Colonel Beaufoy. He employed different needles in his observations, and he found that each gave a variation of its own different from the others. Thus, for example, his observations for the six months of 1815, beginning with April, differ in the quantity of declination from those of the same six months of 1813 and 1814, and they were made with a different needle. To decide this question in a satisfactory manner, it would only be necessary to repeat the observations with a variety of needles employed immediately after each other.

5. Cavallo showed long ago, that when iron is acted upon by diluted sulphuric acid, its magnetic intensity is increased. He put a quantity of iron filings into a watch glass, and placed a magnet at such a distance as scarcely to act upon them. On pouring diluted sulphuric acid on the filings, they were powerfully acted upon by the magnet. This experiment has been lately confirmed and varied by Mr. Ruhland. As electricity is evolved in such abundance during the action of acids on metals, it was natural to expect, considering the striking analogy between electricity and magnetism, that a similar evolution of magnetism would take place when acids are made to act on iron. At the same time the experiments of Cavallo and Ruhland are not quite free from ambiguity; for we may conceive the liquid to act chiefly by giving the filings greater mobility in consequence of the diminution produced in their specific gravity by being plunged in a liquid.

6. There is a curious paper on the magnetism of the earth by Hansten, published in Schweigger's Journal for 1813 (vol. vii. p. 79). He has collected a great many observations on the variation. He shows that the earth must have four magnetic poles. In the year 1769, one of the north magnetic poles was situated in north latitude 70° 17′, and east longitude from Ferro 277° 40·5′. The Siberian north magnetic pole in the year 1805 was situated in north latitude 85° 21·5′, and longitude east from Ferro 133° 49′. In the year 1774 one of the south magnetic poles was in south latitude 71° 26·5′, and 153° 53$\frac{3}{5}$′ east longitude from Ferro; the second in south latitude 77° 16·75′, and 254° 23′ east longitude from Ferro. Hansten's calculations respecting the periodic times

of the revolutions of these magnetic poles are quite erroneous; because he supposes the annual variations equable, which we know not to be the case. Nor have we the least knowledge of the rate at which this annual declination varies, and, of course, cannot pretend to calculate the periods of revolution.

VII. CHEMISTRY.

The annual progress of chemistry being so much greater than that of any of the other sciences, we shall as usual divide it into portions, and arrange the facts which we have to communicate under different heads.

I. GENERAL PRINCIPLES.

1. In the historical introduction to the *Annals of Philosophy* for last year, an account was given of the general principles of the atomic theory. An account was also given of a modification of that theory by Berzelius, in which he substitutes volumes for atoms; supposing all substances in the first place to be in the gaseous state. A very important paper was published in a late number of the *Annals of Philosophy* on this subject. Though the paper in question is anonymous, several circumstances enable me to fix with considerable certainty on the author; but as he chuses to remain for the present concealed, I do not consider myself as at liberty to mention his name. The title of the paper is On the Relation between the Specific Gravities of Bodies in their Gaseous State, and the Weight of their Atoms. (*Annals of Philosophy*, vol. vi. p. 321.) By pointing out this relation, he shows that the two modes of viewing the atomic theory come in fact to the same thing. The mode of determining the specific gravity of some of the gases adopted in this paper deserves attention, as being in all probability capable of giving a more accurate result than the methods hitherto adopted.

The author considers atmospherical air as a chemical compound of one volume of oxygen and four volumes of azotic gas. It is not at all unlikely that this opinion may be correct, though it does not coincide exactly with the analyses of air hitherto made. According to them it is composed of 21 by bulk of oxygen gas, and 79 of azotic gas. This is undoubtedly a near approximation, and if we consider the careless way in which these experiments have been made, and the numerous proofs brought forward by Gay-Lussac to show that gases always combine so that one volume of one gas unites with one, two, three, or four volumes of the other, perhaps we shall be disposed to acquiesce in the conclusion of our author. On that supposition he demonstrates, that if the specific gravity of air be one, then that of oxygen gas is 1·1111, and that of azotic gas 0·9722. This demonstration, however, depends upon two suppositions: 1. That an atom of azote weighs 1·75. 2. That atmospherical air is composed of one atom of oxygen and

two atoms azote. This gives us 100 parts of air composed by weight of

Oxygen ..... 22·222 | Azote........ 77·777

These two suppositions obviously give us the specific gravities above stated by a common algebraic equation; but if we suppose the weight of an atom of azote to be 1·803, as I have stated it in a preceding number of the *Annals of Philosophy*, in that case the specific gravities would turn out differently. Our author, no doubt, borrowed 1·75 as the weight of an atom of azote, from Dr. Wollaston's paper on chemical equivalents. I am disposed to consider it as nearer the truth than my own number.

The specific gravity of hydrogen gas is deduced by calculation from that of ammonia, which has been shown to consist of three volumes of hydrogen and one of azote condensed into two volumes, and the specific gravity of ammoniacal gas is 0·5902. From these data the specific gravity of hydrogen gas is shown to be 0·0694. The only doubt in this calculation is whether the specific gravity of azotic gas, as assumed, be correct. At all events, I have no doubt that it is nearer the truth than the previous results obtained by weighing.

It deserves attention, that if the specific gravity of oxygen, azotic, and hydrogen gases be

Oxygen .... 1·1111 | Azotic ..... 0·9722 | Hydrogen ...0·0694

Then oxygen is just 16 times heavier than hydrogen, and azotic gas 14 times heavier. Water is a compound of 1 hydrogen + 8 oxygen by weight. Supposing it composed of one atom hydrogen and one atom oxygen, then the atom of oxygen is eight times heavier than the atom of hydrogen.

The specific gravity of chloric gas he makes 2·5. That of other substances is founded entirely upon calculation. His method is to find the weight of an atom of the substance in question, and to multiply it by half the specific gravity of oxygen gas; the product gives the specific gravity of the substance, supposing it in the state of gas:

Iodine..........	8·611111	or 124	times that of hydrogen.
Carbon	0·4166	12	ditto.
Sulphur..........	1·1111	16	ditto.
Phosphorus	0·9721	14	ditto.
Calcium	1·3888	20	ditto.
Sodium	1·6666	24	ditto.
Iron	1·9444	28	ditto.
Zinc	2·2222	32	ditto.
Potassium	2·7777	40	ditto.
Barytium	4·8611	70	ditto.

On this table I may make a few remarks. He considers the weight of an atom of iodine 15·5. This agrees very nearly with

the number given by the experiments of Gay-Lussac, namely 15·614.

The number for the weight of an atom of phosphorus is inaccurate. I shall hereafter give the true number deduced from actual experiment.

The weights of the atoms of carbon and sulphur are the same as those I established. I believe them to be nearly correct.

Dr. Marcet's analysis of carbonate of lime is certainly very near the truth. By a very careful analysis I obtained

Carbonic acid	43·2
Lime	56·8
	100·0

This I consider as a still nearer approximation to absolute precision. I believe that in many experiments the weight of the acid has been under-rated, because a portion of it remained in the vessel in which the carbonate was dissolved.

I do not consider the mode adopted by the author for determining the weight of an atom of sodium, iron, zinc, &c. as susceptible of much precision. It consists in determining how much of these bodies is soluble in a quantity of muriatic acid capable of dissolving a known portion of carbonate of lime. The sources of inaccuracy are so numerous that they cannot be all guarded against.

2. It has been long known to philosophers, that the bulk of air, and of all gases, is inversely as the pressure. We are in the habit in this country of ascribing the discovery of this law to Boyle. The French, on the contrary, ascribe it to Mariotte. I do not know which of these philosophers was really the discoverer of this law, because I have not in my possession the original dissertation of Mariotte, and do not know when it was published; but if it was published in 1666, as I have some reason for believing, the priority certainly belongs to Mariotte: for Boyle's experiments were not published so early. When a body is in the gaseous state, the particles are at such a distance, that the attractions and repulsions which they possess have no sensible action, so that the distance is regulated solely by the repulsion produced by caloric, and by the external pressure. This is evident; for if a volume of oxygen gas be mixed with a volume of hydrogen gas, the mixture constitutes two volumes. Yet we know that there is an attraction between these two gases. Therefore, if the particles were within the sphere of each other's attraction they would approach each other, and the bulk would be less than two volumes. M. Ampere has demonstrated that the resistance to external pressure made by any gas is directly as the number of particles of the gas in a given bulk. This is precisely the law of Mariotte.

3. *Adhesion* exhibits the characteristic marks of chemical affinity, and ought therefore to be considered as a particular case of

the action of that power. The different experiments on adhesion made by Brook Taylor, Morveau, and Achard, are well known, together with the defects of these experiments, pointed out long ago by Dutour. The subject still requires a good deal of experimental elucidation. Mr. Ruhland (Schweigger's Journal, ii. 146) has lately made a number of very curious experiments on adhesion, which deserve to be generally known. I shall give a short abstract of them here, leaving out his explanations, which do not appear to me to throw much light upon the subject.

He found a weight of 46 grains necessary to separate a watch glass from the surface of mercury. On heating the watch glass, 58 grains were requisite to separate the two surfaces. When the watch glass and mercury were allowed to acquire the same temperature, the same weight (or rather less) as at first, was sufficient to separate the two surfaces. The same experiment succeeded when marble was substituted for the watch glass.

Mr. Ruhland found, as indeed was known before, that when the smooth surfaces were rubbed against each other, their adhesion was very sensibly increased. I suspect in these cases the agency of electricity: the two surfaces probably acquire different states of excitement.

He took a small glass plate and made it adhere to the surface of a little mercury placed in a watch glass. The weight necessary to separate the two surfaces was three grains. The mercury was now brought in contact with a little nitric acid; of course an effervescence took place, and some nitrate of mercury was formed. The weight now necessary to separate the two surfaces was seven grains. The same experiment was repeated various ways, always with a similar result. Mr. Ruhland supposes that a thin film of nitrate of mercury gets between the metal and the glass.

The following table exhibits the weights which were found necessary to separate equal surfaces of the bodies, stated in the table, from different liquids.

Water, twice distilled.

Zinc	77 grs.	Tallow	76 grs.
Wax	79	Lead	74
Sealing wax	70·5	Marble	77
Glass	75·5	Sulphur	80

Oil of Almonds.

Marble	50 grs.	Wax	56 grs.	Tallow	54 grs.

Alcohol, diluted with thrice its Weight of Water.

Marble	45 grs.	Wax	50 grs.	Tallow	49 grs.

Twenty Grains concentrated Nitric Acid in two Ounces of Water.

Glass	64 grs.	Wax	70 grs.
Lead	67	Tallow	69
Sulphur	65		

Twenty Grains concentrated Sulphuric Acid in two Ounces Water.

Glass	75·5 grs.	Wax	79 grs.
Sulphur	75	Tallow	77
Lead	80		

Concentrated Solution of Potash.

Zinc	65 grs.	Tallow	66 grs.
Sulphur	67	Glass	64
Wax	69		

The effect of the addition of a little acid or salt to water was remarkable. Two ounces of pure water were employed. Plates of zinc and glass required the following weights to separate them from the liquid:

Zinc	78 grs.	Glass	76 grs.

Six grains of concentrated nitric acid being added to the water, the weights required were as follows:

Zinc	74 grs.	Glass	69 grs.

In another experiment the weights necessary to separate the zinc and glass from pure water were as follows:

Zinc	78 grs.	Glass	76·5 grs.

Six grains of concentrated sulphuric acid being added, the weights became

Zinc	75 grs.	Glass	71 grs.

These are the principal experiments. I pass over some others, the chief object of which is to point out the errors liable to be committed in such experiments.

4. *Fermentation* is one of those mysterious processes which chemistry has not yet been able to explain. On that account every fact relating to it is of importance; for it is only by an accumulation of facts that we can expect to throw light upon this obscure but most important process. I shall therefore state here the result of some experiments made upon yeast by Dobereiner. They are not of much importance; but still they are worth knowing. (Schweigger's Journal, xii. 229.) He obtained his yeast from ginger beer. To render it quite pure he washed it in four times its weight of alcohol. The yeast was not altered in its appearance; but it was tasteless, and was incapable of producing fermentation in a weak solution of sugar in water. When sugar is dissolved in water in such quantity as to reduce the whole to the consistence of a syrup, it may be mixed with yeast, and kept for any length of time without fermentation taking place; but the moment the mixture is sufficiently diluted with water fermentation begins.

5. The absorption of the gases by solid substances is a subject of considerable importance which had occasionally engaged the attention of chemical philosophers. Delametherie, Morozzo, Rouppe, and Von Norden published successively a variety of experiments on

the subject; but they confined themselves chiefly to the action of charcoal, and their experiments were neither sufficiently varied nor precise to throw much light on the subject, far less to enable us to form a theory explaining the nature of that absorption. But M. de Saussure of Geneva, one of the most accurate chemists of the present day, has lately turned his attention to the subject, and published a most elaborate set of experiments. He did not confine himself to charcoal, but tried a number of other solid substances, and varied his experiments so much as to enable him to give us a very satisfactory theory of these absorptions. I first saw this important paper in Gilbert's Annalen der Physik, and immediately translated it for the *Annals of Philosophy*. It lay some time by me before I could find room to insert it; but its importance was such that I was unwilling to withhold it from my readers, and therefore substituted it in place of the customary biographical article with which the numbers of the *Annals of Philosophy* usually commence. It will be found in the fourth and fifth numbers of our sixth volume.

The general result of De Saussure's experiments leads to the conclusion, that the absorption of gases by porous solid bodies depends upon the same cause as the capillary attraction of liquids. Chemical affinity doubtless has its effect, as it has also upon capillary attraction. Charcoal, meerschaum, ligniform asbestus, rock cork, hydrophane, quartz, sulphate of lime, mineral agaric, hazel-wood, mulberry, fir, linen thread, wool, and raw silk, were the solid bodies employed, and all of them have the property of absorbing gases.

Charcoal absorbs the most gases, and the proportions absorbed by the other bodies are nearly in the order in which they have been named. Each of these substances absorb a determinate quantity of every particular gas; but the order is not the same for the different solid bodies indicating the action of chemical affinity. Thus charcoal absorbs more nitrous oxide than carbonic acid gas; but meerschaum absorbs more carbonic acid gas than nitrous oxide. The following table exhibits the nnmber of volumes of the different gases absorbed by dry box-wood charcoal.

	Volumes.		Volumes.
Ammoniacal gas	90	Olefiant gas	35
Muriatic acid	85	Carbonic oxide	9·42
Sulphurous acid	65	Oxygen	9.25
Sulphureted hydrogen	55	Azote	7·5
Nitrous oxide	40	Oxy-carbureted hydr.	5
Carbonic acid	35	Hydrogen	1·75

Water diminishes the power of solid bodies to absorb gases. And when a solid body is saturated with a gas, the addition of water disengages a portion of this gas. During the absorption of gases by solid bodies, heat is disengaged, owing obviously to the condensation of the gas in the pores of the solid body. Thus the density

of ammoniacal gas absorbed by box-wood charcoal is increased 90 times. When the gases are rarified the solid bodies absorb a greater bulk of them than when they are of the density produced by the pressure of the atmosphere. This might have been expected, supposing the absorption to be occasioned by an attraction between the solid body and the gas, as is obviously the case. When a piece of charcoal, saturated with one gas, is put into another gas, it allows a portion of the first gas to escape, and absorbs a portion of the new gas. The quantity of gas, thus expelled, is always the greater the more there is an excess of the gas which produced it. Two gases united by absorption in charcoal, often experience a greater condensation than each would in a separate state. For example, the presence of oxygen gas in charcoal facilitates the condensation of hydrogen gas: the presence of carbonic acid gas or of azotic gas facilitates the condensation of oxygen gas: and that of hydrogen gas the condensation of azotic gas. Yet no perceptible combination takes place between the gases thus absorbed together.

M. de Saussure likewise examined the absorption of the different gases by liquids, in order to determine whether Dalton's theory was consistent with the phenomena. Most of my readers, I presume, know that, according to Mr. Dalton, the absorption of gases by liquids is merely mechanical, and not influenced by chemical affinity. The quantity absorbed, according to him, is either $(\frac{1}{1})^3$, $(\frac{1}{2})^3$, $(\frac{1}{3})^3$, or $(\frac{1}{4})^3$, that is to say, liquids absorb their own bulk of carbonic acid gas, sulphureted hydrogen gas, and nitrous oxide; $\frac{1}{8}$th of their bulk of olefiant gas; $\frac{1}{27}$th of their bulk of oxygen and nitrous gas; and $\frac{1}{64}$th of their bulk of azote, hydrogen, and carbonic oxide. M. de Saussure found the absorption of different gases by water and alcohol as in the following table.

	100 volumes of Water.	100 volumes of Alcohol. Sp. Gr. 0·84.
	Volumes.	Volumes.
Sulphurous acid gas	4378	11577
Sulphureted hydrogen	253	606
Carbonic acid	106	186
Nitrous oxide	76	153
Olefiant gas	15·3	127
Oxygen gas	6·5	16·25
Carbonic oxide	6·2	14·5
Oxy-carbureted hydrogen	5·1	7·0
Hydrogen	4·6	5·1
Azote	4·1	4·2

From this table we see that one part of Dalton's theory, namely, that gases are absorbed in the same proportion by all liquids, is inaccurate; for alcohol obviously absorbs a much greater proportion of the gases than water. The proportions of sulphureted hydrogen, carbonic acid, and nitrous oxide, absorbed by water, are so different, that Mr. Dalton's opinion, that water absorbs exactly its

own bulk of each must be erroneous. Finally, M. de Saussure found that different liquids absorb the gases in different orders: thus naphtha absorbs more of olefiant gas than of nitrous oxide, while essential oil of lavender absorbs more of nitrous oxide than of olefiant gas. This can only be ascribed to the agency of chemical affinity. Finally, he ascertained by experiment, that Mr. Dalton's opinion respecting the quantity of gas disengaged, when water saturated with one gas is brought into contact with another gas, is inaccurate. It would appear, therefore, from these experiments of De Saussure, that Mr. Dalton's theory is errroneous in every particular.

II. HEAT.

No new discovery respecting heat, so far as I know, has been made during the course of last year. Mr. Davenport, has, however, written a very complete refutation of some objections that had been started against M. Prevost's theory of radiant heat; and M. Prevost himself has given us a very short but clear outline of his theory, which has been adopted, I believe, by almost all the chemical philosophers of the present day. Both of these papers will be found in the *Annals of Philosophy*; the first in the fifth, and the second in the sixth volume of that work. M. Prevost's outline of his theory being very short, it may be expected that I should give it here. It is in substance as follows:

1. Heat is a discrete fluid, every particle of which moves rapidly in a straight line. These particles go one in one direction and another in another; so that every sensible point of the hot space is a centre from which depart, and to which arrive, rows of particles or calorific rays.

2. Every calorific ray, which a body sends by emission or by reflection, only replaces another ray, which would take the same direction if the body were withdrawn. This is to be understood of a hot place where heat radiates. If the intercepting body is of the same temperature with the place, the ray which it replaces is equal to itself; if not, this ray or row of particles, is more or less abundant in heat.

3. A reflector in a place of uniform temperature sends neither more nor fewer calorific rays than another body.

4. It follows from this: 1. That in a place of uniform temperature, a reflector of whatever form does not affect a thermometer subjected to its influence. 2. That if it reflect rays emanated from a body more or less hot than the place, it will raise or depress respectively the thermometer subjected to its influence.

In the last volume of the Edinburgh Philosophical Transactions, published during the summer of 1815, there is a paper by Dr. John Murray, Lecturer on Chemistry in Edinburgh, on the Diffusion of Heat at the Earth's Surface. This ingenious gentleman had started the following objection to the Huttonian theory, which he considered as fatal to its truth. If a central fire existed in the earth, as Hutton supposes, from the very nature of heat it could

not remain at the centre, but must diffuse itself equally through the whole globe; so that in time the surface of the earth would become as hot as the centre. To this Mr. Playfair answered, that the alleged equalization would undoubtedly take place, provided heat did not make its escape from the surface of the earth; but if the heat (as was probable) escaped from the surface of the earth as fast as it was conveyed to it from the centre, then no accumulation or increase of temperature would take place. The principal object of the paper in question, is to show that no such escape of heat takes place from the surface of the earth; that nature has guarded against it effectually by the constitution of the atmosphere; that heat, in consequence, is constantly accumulating on this globe of ours; and that the time will come, when the whole surface of the earth shall have acquired the same temperature. According to this notion the polar regions are becoming annually warmer, and will in time equal the heat of the torrid zone.

But I conceive that our ingenious author has committed an oversight, by greatly underrating the effect of the radiation of heat. Whoever will peruse with attention Dr. Wells's admirable Essay on Dew, will be satisfied that this radiation, even in cold weather, is very considerable. On clear nights he often found the grass 13° or 14° colder than the atmosphere. There is no reason for supposing that heat radiated in this manner is intercepted by the atmosphere. As we have no evidence that the polar regions are warmer at present than they were some thousand years ago, I do not see why we should believe that any such amelioration has taken place. Indeed the evidence is rather on the contrary side; for North Greenland, which formerly was accessible and even colonized by the Danes, has been for ages blocked up with ice, so that we are ignorant whether the wretched inhabitants still struggle with their situation, or have perished from want of food and by the inclemency of the climate. Mr. Scoresby has ascertained that the mean temperature of latitude 78°, on the coast of Spitzbergen, is only 18°, instead of 34°, as calculated by Mr. Kirwan; and he thinks it demonstrable, that at the pole the mean temperature will not exceed 7° or 8°. It is much more probable that the heat thrown from the surface of the earth by radiation is nearly equivalent to what is thrown into it by the solar rays, and that the mean temperature of the globe remains nearly stationary. We have other proofs against the existence of a central fire, which appear quite conclusive. The mean temperature of mines is always found to be the mean temperature of the country in which the mine in situated. No increase of temperature has ever been observed to take place as the mine increases in depth, yet this ought undoubtedly to happen on the supposition of the existence of a central fire, even according to the reasoning of Mr. Playfair himself.

III. SIMPLE SUPPORTERS.

The simple supporters of combustion, at present known, amount

to three; namely *oxygen*, *iodine*, and *chlorine*; and, if Ampere's hypothesis respecting *fluorine*, so ably supported by Sir Humphry Davy, be correct, it will constitute a fourth. Many important facts respecting these bodies have been lately ascertained. I shall state the principal of them in this place.

1. *Oxygen*. This substance was raised by Lavoisier to a very high rank among chemical substances. He considered it as the acidifying principle, as the only supporter of combustion, and as capable of uniting with and modifying all other simple bodies. The modern discoveries in chemistry have deprived oxygen of a good deal of its dignity. Davy has shown that it forms alkalies as well as acids, and that many acids exist which contain no oxygen. It is not therefore the acidifying principle. This indeed is a doctrine which was all along maintained by Berthollet, whose sagacity in many points of chemical theory deserves the highest admiration.

Oxygen has lost likewise the property of being the only simple supporter of combustion. For chlorine possesses that property perhaps in a greater degree than oxygen; with this curious exception, that charcoal will not burn in it nor unite with it. Iodine is certainly a much less perfect supporter of combustion, since the only body hitherto observed to burn in it is potassium. It is amusing to observe the awkward attempts of the French chemists to preserve for oxygen the exclusive privilege of being the only simple supporter of combustion. According to them *combustion* in the chemical sense of the word is very different from the meaning which it bears among the vulgar. Nothing, says Thierry, is more similar to combustion than what takes place when phosphorus is introduced into chlorine gas. We have flame and the phosphorus disappears. Nothing, on the other hand, is more unlike combustion, than the rusting of iron in a damp place. Yet the first, he informs us, is not a real combustion, while the second is. (Annales de Chimie, xciii. p. 53.) It is surprising that these gentlemen do not perceive that they are merely altering the meaning of a word, which has been known and understood ever since mankind were acquainted with fire. The burning of phosphorus in chlorine would be called *combustion* by every person of common sense who witnessed the phenomenon. Nor is there any thing in the chemical meaning of the term, which is incompatible with its application to this and many other similar cases. The rusting of iron in a damp place would never be called combustion either by the vulgar or by chemists, who considered the case with attention. It consists merely in the transfer of the oxygen of water to the iron. Thenard and Gay Lussac have arranged *chlorine* and *iodine* among combustible substances, merely because they have the property of combining with oxygen. If they had placed these bodies in a class by themselves, their conduct might have been excusable; but to call them combustible is absurd; because nothing similar to combustion, in any sense of the word, takes place when they unite with oxygen. The union cannot be directly accomplished, and is

far from intimate. Why should the supporters of combustion not have the property of uniting with each other? It has been long known that the simple combustibles have that property. Sulphur unites to copper with such violence as to produce both light and heat in abundance; yet nobody on that account has thought proper to class sulphur among the supporters of combustion. Neither is it a sufficient reason to class chlorine and iodine along with sulphur, that all the three unite with hydrogen and form an acid.

The only exclusive privilege which remains to oxygen is, that it alone, or its compounds, are fit for the *respiration* of animals, and necessary indeed to preserve life. The breathing of the other supporters of combustion is almost instantly fatal to animal life.

2. *Chlorine* is now pretty generally admitted to be a simple supporter of combustion. Almost the only chemist of eminence who adheres to the old opinion is Berzelius. His opposition is founded on the supposed inconsistency of Davy's theory with the chemical canons, which he has established by a vast number of uncommonly accurate analyses. But this inconsistency, I am persuaded, he will find on a closer examination to vanish entirely. If this were the proper place, I think I could show that the doctrine of Davy and the canons of Berzelius agree perfectly with each other.

In Schweigger's Journal for May 1815 (vol. xiii. p. 72) there is a long paper by Professor Hildebrandt, stating several objections to Davy's theory of chlorine. I was extremely surprised on reading this paper to find that all the objections it contained had been examined and answered long ago, and that all of them were founded on mistakes. Chlorine, he says, converts nitrous gas into nitric acid, and therefore it must contain oxygen. This was the first experiment that I tried when Davy published his theory. I found that the change here stated actually took place; but on examining my chlorine it was mixed with common air; and upon preparing pure chlorine I found that it produced no change on nitrous gas. Davy afterwards made the same experiment and published it, and the fact is now well known to all chemists of precision. Another objection is, that when common salt is decomposed by the galvanic battery the chlorine appears at the positive wire. This, so far from being an objection, is a strong argument in favour of Davy's theory. Oxygen and iodine are likewise attracted by the positive pole; so should chlorine, if it be a simple supporter of combustion.

Another objection is that, when metals are burnt in chlorine gas, they are converted into oxides. The fact is not so, unless water be present in the vessel; they are converted into *chlorides*, a variety of which have been describet by Dr. John Davy. The other objections of Hildebrant are all of a similar nature, and do not appear to me to be worth mentioning, as they have all been refuted long ago.

It is amusing to observe the efforts which the French chemists have made to deprive Davy of the honour of this theory. There is

a long paper by Bidault-de-Villiers in the Annales de Chimie (vol. xciii. p. 32) to prove that Scheele did not consider chlorine as a simple substance. The proof is most extraordinary. Scheele's opinion was not adopted by chemists in general, not even by Davy himself; therefore Scheele did not maintain it. The very name, *dephlogisticated muriatic acid*, given to chlorine by Scheele, shows us what his opinion was. Phlogiston in Scheele's opinion, as every body knows, was hydrogen gas. If, therefore, chlorine was muriatic acid deprived of hydrogen, it is obvious that he must have considered muriatic acid as a compound of chlorine and hydrogen; and accordingly this opinion was maintained by Kirwan in his essay on phlogiston, on the authority of Scheele. Scheele says, in his essay on manganese, "muriatic acid deprived of phlogiston, which is one of its constituent parts." (*L'acide muriatique depouillé du phlogistique qui est une de ses parties constituantes.*) * I cannot conceive any thing more explicit than this.

There can be no doubt that it was the experiments of Gay-Lussac and Thenard, published in their Recherches Physico-Chimiques, that led Davy to form the new theory. So far their merit is conspicuous. But as they did not adopt the new theory in that work, but argued against it, nothing can be more ridiculous than to claim it after it has been established by another. If Gay-Lussac always maintained it, as he informs us, but was prevented from publicly embracing it by the authority of Berthollet, we may pity his pusillanimity, but cannot on that account admit his claim as the first propagator of a theory, which he publicly opposed. As to M. Ampere, his posthumous claim cannot be maintained, as he published nothing whatever on the subject.

Gay-Lussac has lately added an important fact to what was previously known of chlorine. He has shown that it combines with more than its weight of oxygen gas, and forms an acid, to which he has given the name of *chloric acid.* He has pointed out a method of obtaining this acid in a separate state, and has shown that it is a constituent of the salts called formerly *hyper-oxymuriates*, to which henceforth the name of *chlorates* must be given. Vauquelin has published an account of the properties of these salts.

Sir Humphry Davy has lately discovered a new gaseous compound of chlorine and oxygen, which does not seem to possess acid properties. It is obtained by reducing chlorate of potash (*hyper-oxymuriate of potash*) to a powder, making it into a solid paste with sulphuric acid, and exposing it in a small retort to a heat not so high as 212°. A gas is obtained, which is the substance in question. It has a much more intense greenish yellow colour than chlorine; does not act upon mercury; but is rapidly absorbed by water. The taste of it is astringent, and it is very corrosive. When phosphorus is introduced into it an explosion takes place, and the

* Memoires de Chymie de M. C. W. Scheele, i. p. 69. I quote the French translation, that the French chemists may consult the passage, though this deprives me of Kirwan's notes.

combustible burns in the liberated gases with great brilliancy. When heated, it explodes with more violence than euchlorine; and two measures of it seem to be converted into three, two of which are oxygen and one chlorine. This approaches the supposition that the compound is formed of

Chlorine .... 1 atom	Oxygen .... 4 atoms

But it is obvious that if the weight of an atom of chlorine be 4·4, and that of an atom of oxygen 1, the two gases never can combine in one volume chlorine and two oxygen. Accordingly Davy found less than one volume of chlorine to two of oxygen. If we suppose 0·9 of a volume of chlorine to two volumes of oxygen, which was what Davy actually found, the substance will then be a compound of one atom chlorine and four atoms oxygen.

Thus we have three compounds of chlorine and oxygen, namely,

	Chlorine.	Oxygen.
Euchlorine, composed of......	1 atom	+ 1 atom
New gas	1	+ 4
Chloric acid.................	1	+ 5

It will be necessary to apply systematic names to these substances. I would recommend to Sir H. Davy to employ the Greek numerals in his nomenclature, because they apply to any quantity of compounds of the same constituents, without occasioning any confusion. Thus his *euchlorine* might be called *protochlorous oxide*; his new gas, *deutochlorous oxide*; and Gay-Lussac's *chloric acid*, *perchloric acid*. These names would be distinct, and much more easily remembered than arbitrary terms given without any regard to system. We may affect to disregard system in our names, but the affectation is improper. Without attending to it, the science of chemistry becomes a mere mass of confusion. When our chemical theories are shown to be erroneous, let the names be altered. This does no harm, if it is not in reality attended with good.

3. *Iodine.*—A great number of papers on iodine have been published during the course of the year 1815; but after the very complete treatise on that subject by Gay-Lussac, inserted in the fifth and sixth volumes of the *Annals of Philosophy*, these papers cannot be expected to exhibit much novelty. The following are the only new facts that I have observed in them.

The *iodide* of gold is a white powder. Uranium is precipitated by a hydriodate of a dirty dark colour. Link, Fischer, and Steffens. (Schweigger's Journal, vol. xi. p. 134.)

The iodide of antimony has a dark red colour, and is soluble in water. The iodide of bismuth is orange. Its solution is not precipitated by potash. The iodide of arsenic is dark purple-red, and possesses acid properties. The iodide of tellurium is dark purple-red, and forms a colourless solution with potash. Ruhland. (Ibid. p. 139.

Dr. Wollaston has determined the figure of the crystal of iodine to be a rhomboidal octohedron, whose axes are to each other as the

numbers two, three, four. (See *Annals of Philosophy*, vol. v. p. 237.)

Gaultier de Claubry and Stromeyer have ascertained that starch is the most delicate re-agent for detecting the presence of iodine. The iodine must be uncombined. Starch does not detect iodine in a solution containing hydriodic acid or iodic acid. But if an acid be poured in so as to disengage the iodine, the starch shows the presence of that substance by the indigo-blue colour which it assumes. (Gilbert's Annalen, vol. xlix. p. 146; and Ann. de Chim. vol. xciii. p. 85.)

M. Gaultier de Claubry has analyzed sea water and several fuci from the English channel. He could detect no iodine in sea water; but he found it in the following sea plants: fucus saccharinus, fucus digitatus, fucus vesiculosus, fucus siliquosus, fucus filum. (See Ann. de Chim. vol. xc. p. 75, 113.)

I have been informed that Mr. Smithson Tennant before his death succeeded in detecting iodine in sea water; but I know nothing respecting the method which he followed in his investigation.

Sir Humphry Davy has discovered a solid combination of iodine and oxygen. I place it here because the discoverer does not consider it as acid unless it be combined with water, though I entertain a different opinion from this ingenious chemist. It is obtained by exposing iodine to the action of euchlorine gas. The gas is absorbed, and a solid substance formed consisting of two compounds; the first, a combination of chlorine and iodine; the second, of oxygen and iodine. By the application of a gentle heat, the first compound is driven off, and the second remains. Sir H. Davy gives it the name of *oxiodine*; but perhaps the term *oxiodic acid* would be more proper. It is white, and semi-transparent; has no smell; but a strong astringent sour taste. It sinks rapidly in sulphuric acid. A heat rather below 600° decomposes it. According to Davy's experiments, it is composed of

Iodine	81·28	...... 1 atom
Oxygen	18·72	 4
	100·00	

This compound is deliquescent. It is very soluble in water. The solution reddens vegetable blues, and then destroys them. It acts upon all the metals, and combines with alkalies, earths, and metallic oxides. It unites likewise with the acids, and forms with them solid compounds, which possess remarkable properties.

IV. SIMPLE COMBUSTIBLES.

1. *Hydrogen.*—One of the most important experiments which can well be made in the present state of the science of chemistry is related in the *Annals of Philosophy*, (vol. vi. p. 234) on the authority of Van Mons. Dobereiner introduced a globule of mercury into a vessel of water, and placed it near the negative wire of a galvanic battery. Oxygen gas was given out from the positive wire; but no gas whatever was extricated from the negative wire. The

globule of mercury was however attracted by it, and was gradually converted into an amalgam. Hence it would seem that hydrogen has the property of forming an amalgam with mercury. If so, it must be a metallic body; at least if the opinion universally admitted at present be true, that mercury amalgamates only with metals. I am disposed to admit the truth of this experiment, though I have not repeated it, because some years ago I remember that Sir H. Davy very nearly hit upon it. He exhibited the evolution of oxygen gas at the positive pole without the extrication of any gas whatever at the negative wire; but the quantity of mercury which he used being very considerable, no sensible amalgam was formed; but if hydrogen be a metal, we have a simple explanation of the amalgam formed by means of ammonia or sal-ammoniac and mercury when acted upon by the galvanic battery. The speculations on the presence of oxygen in ammonia, as far as they depend upon that experiment, would be refuted; while a good deal of the most plausible part of the reasoning respecting the composition of azote would likewise be destroyed. It becomes, therefore, of the utmost consequence to verify or refute this fundamental experiment of Dobereiner.

2. *Charcoal.*—As to Dobereiner's metallization of charcoal, mentioned also in the *Annals of Philosophy* (vol. v. p. 237), I have not yet had an opportunity of judging how far his experiments are correct. There is an account of these experiments in the first volume of Dobereiner's Chemistry; but as I have not yet seen that book, I do not know how they were made, nor what degree of confidence can be put in them. At present I own I am not disposed to admit the truth of the opinion.

Dobereiner has published a set of experiments in order to show that charcoal has the property of purifying air, and of freeing it from those offensive fumes with which it is often contaminated. Nothing more is necessary than to put the charcoal into the contaminated air, and to allow it to remain for a certain time. It imbibes the noxious fumes, and deprives the air of all smell. In this way he freed air from the fumes of tobacco smoke, of asafœtida, &c. When water is present at the same time with the charcoal, the air is purified the sooner.

3. *Cyanogen.*—Gay-Lussac has lately discovered a new gaseous substance, to which he has given the name of *cyanogen,* because it constitutes the basis of *prussic acid.* It is obtained by exposing dry *prussiate of mercury* to heat in a retort. The gas speedily passes over, and must be received over mercury. It possesses the following properties:—

It is colourless; has a very pungent, but quite peculiar smell; its specific gravity is 1·8011; it burns with a blue flame, requires twice its volume of oxygen gas for combustion, and leaves as a residuum one volume of azotic gas, while two volumes of carbonic acid gas are formed. Hence it is composed of

Carbon	46·1		2 atoms
Azote	53·9		1
	100·0		

Water absorbs $4\frac{1}{2}$ times its bulk of this gas; alcohol, 23 times its bulk; and sulphuric ether and oil of turpentine, at least as much as water. It combines with potassium, mercury, and several other bodies, and forms a set of bodies to which Gay-Lussac has given the name of *cyanurets*. The term *cyanides* would have been more proper; for cyanogen in these combinations acts a part exactly analogous to that of chlorine and iodine, which are obviously supporters of combustion. Therefore the names of all their combinations with combustibles ought to be analogous to those of the compounds of oxygen and the same bodies; that is to say, they should end in *ide*. What is called prussiate of mercury is a *cyanide* of mercury.

V. METALS.

1. *Platinum.*—Dobereiner has shown, as Mr. Smithson Tennant had done long ago, that when nitre or nitrate of soda is melted in a platinum crucible, a portion of the platinum is oxidized. But none of the other nitrates seem to possess this power. He tried nitrate of barytes, nitrate of strontian, and nitrate of lime, without obtaining any oxide of platinum. The oxide obtained had a reddish-brown colour, almost the metallic lustre, and was completely soluble in sulphuric acid.

2. *Copper.*—When copper is dissolved in nitric acid, the solution is at first *green* and *muddy*; by degrees a yellow precipitate falls to the bottom, and the liquid becomes transparent and blue. Professor Hildebrant has examined this yellow precipitate, and concludes from his experiments that it is an oxide of copper containing more oxygen than the black oxide of copper. The fact would be curious and important, if this be really the case; but Hildebrandt's experiments do not appear to me quite satisfactory. They would require repetition, and several points ought to be determined with more precision than he has done. (See Schweigger's Journal, vol. xi. p. 169.)

3. *Iron.*—Dr. Henry has observed a curious effect produced upon cast-iron when left in contact with solutions of muriate of lime or muriate of magnesia. Most of the iron was removed, the specific gravity of the mass was reduced to 2·155, and what remained consisted chiefly of plumbago, and the other impurities present in cast-iron. (See *Annals of Philosophy*, vol. v. p. 66.)

4. *Nickel.*—Lampadius has given us a set of experiments on pure nickel, which have been inserted in the *Annals of Philosophy* (vol. v. p. 61). Its magnetic energy compared with that of iron he found as 35 to 55. He alloyed it with gold, platinum, copper, and iron. He did not succeed in uniting it with silver. It combined readily with phosphorus and sulphur.

5. *Zinc.*—Vogel has published a set of experiments on zinc. The following are the results which he obtained. The black powder which remains when zinc is dissolved in sulphuric acid consists of charcoal, iron, and sulphate of lead. He could not obtain any more than one oxide of zinc, and therefore denies the existence of the supposed protoxide of zinc obtained by Clement and Desormes and by Berzelius. The flowers of zinc of the apothecaries always contain less or more carbonic acid. There exists a subsulphate of zinc, which is sparingly soluble in boiling water.

6. *Arsenic.*—A great number of experiments have been lately made on the solubility of white oxide of arsenic in water. Those of Klaproth have been published in a preceding volume of the *Annals of Philosophy*. A still more complete set of experiments were afterwards published by Professor Bucholz. But the most elaborate of all are those of Mr. Fischer, a public teacher in the University of Breslau. They occupy no less than 40 pages in Schweigger's Journal (vol. xii. p. 155). The results at which he arrived are as follows.

White oxide of arsenic is insoluble in water. Its solution takes place only when it is changed into an acid by combining with a greater proportion of oxygen, which it absorbs at the expense of the undissolved portion. Hence the reason why the undissolved portion loses its white colour, and becomes of a dirty yellow. This change takes place at all temperatures between that of the common temperature of the atmosphere and that of boiling water. 12·343 parts of boiling water dissolve one part of this substance; between the temperatures of 122° and 144°, 22 parts of water are necessary to dissolve one part of this substance; between the temperatures of 66° and 77°, 50 parts of water are necessary; while between the temperatures of 45° and 50°, 66·6 parts of water are necessary to produce this solution. To obtain the solutions in these proportions, a very long action of water upon white arsenic is necessary.

7. *Tungsten.*—A set of experiments on tungsten by Bucholz, made some years ago, but promised in the historical introductory discourse of last year, have been published in the *Annals of Philosophy* (vol. vi. p. 198). He shows that the methods hitherto followed to obtain tungstate of ammonia do not furnish that salt in a state of purity, and that the impure salt does not yield the metal when smelted in the usual way, but runs into a slag. This is probably the reason why so few chemists have succeeded in obtaining tungsten in the metallic state. Bucholz obtained it in that state. He was not able to fuse it; but he confirmed the previous experiments of the Elhuyarts and Allen and Aiken respecting the great specific gravity of this metal. He found its specific gravity 17·4, which is a mean between the results obtained by the Elhuyarts (17·6) and Allen and Aiken (17·2).

8. In a late number of the *Annals of Philosophy* (vol. vi. p. 75) it was stated that Brugnatelli had formed an amalgam by exposing

mercury to the beautiful purple fumes which exhale when indigo is heated. Hence he concluded that indigo contains a metallic basis. This experiment has been confirmed by Dobereiner. He heated together 30 grains of the finest Guatimalo indigo and 10 grains of mercury in a porcelain dish (constantly triturating the mixture) till the purple fumes from the indigo began to appear. He obtained a solid amalgam of mercury, which, when heated, exhaled the purple fumes of indigo. When digested in sulphuric acid, it communicated a dark blue colour. When put into nitrate of silver, crystals of silver were speedily deposited in the form of an artichoke. These, being digested in sulphuric acid, coloured it blue, indicating an alloy of silver and the metal of indigo.

Dobereiner conceives that many other vegetable metals exist. If these experiments should be confirmed, I think it is high time for chemists to examine whether the mere property of combining with mercury without destroying the metallic lustre of that body be sufficient of itself to constitute a metal. This opinion seems to have been adopted on too slight grounds. If Ruhland's statement (Schweigger's Journal, xiii. p. 359), that mercury amalgamates with sulphureted hydrogen and phosphureted hydrogen gases, be true, it seems obvious that the mere amalgamation with this liquid is not of itself sufficient to prove the metallic state of a body.

VI. ACIDS.

1. *Sulphuric Acid.*—Professor Link, of Breslau, has published a set of experiments on the action of sulphuric acid on vegetable substances; but I do not perceive any new facts in these experiments, except that when this acid is digested on sugar or gum a quantity of malic acid is formed.

The general opinion of chemists for some time past has been that the fuming sulphuric acid of Nordhausen is an acid free from water. This opinion has been lately verified by Dobereiner. He made water absorb 58 grains of the fuming acid, and precipitated the liquid by barytes-water. The sulphate of barytes obtained weighed 170 grains. Now if we allow this salt to contain 34·5 per cent. of sulphuric acid, it is obvious that 170 grains of it contain 57·75 grains of sulphuric acid; but this is almost exactly the quantity of fuming acid absorbed by the water.

2. *Chloric Acid.*—Vauquelin has obtained this acid in a state of purity. He formed chlorate of barytes by the method of Chenevix, taking care, however, not to employ any acetic acid to facilitate the action of the phosphate of silver. This acid possessed the following properties:—

It is colourless; its taste is acid and astringent; its odour, when concentrated, is somewhat pungent.

It reddens infusion of litmus; it does not precipitate silver, lead, nor mercury, from their solution in nitric acid; it does not precipitate gelatine, notwithstanding its astringent taste, though chlorine possesses this property.

When paper stained with litmus is left in contact with it for some days, the colour of the litmus is destroyed. When this acid is heated, the greatest part of it is volatilized, though a portion of it is decomposed into oxygen and chlorine. Muriatic acid, sulphureted hydrogen, and sulphurous acid, decompose chloric acid, and develope chlorine, provided they be not added in excess. With muriatic acid, water is formed, and the two acids are converted into chlorine. With sulphureted hydrogen, sulphur is deposited, water formed, and chlorine disengaged. The sulphurous acid is converted into sulphuric by taking oxygen from the chloric acid, which in consequence is converted into chlorine.

3. *Acetic Acid.*—It is the custom in Germany to distil vinegar in stills, whose heads and conducting pipes are composed of English tin. Professor Pfaff, of Kiel, has shown that vinegar distilled in this way holds a little tin in solution. Accordingly, when sulphureted hydrogen is mixed with it, a dark brown precipitate appears. This precipitate had been ascribed to lead; but Pfaff ascertained that it is always owing to tin, even when soft solder is present, which contains a notable proportion of lead.

4. *Prussic Acid.*—From a set of experiments published by Dobereiner, it would appear that when an alkali is heated with pure charcoal, ammonia is always formed; but when, besides the charcoal, iron, or any substance containing iron, is present, prussic acid, or rather ferrureted chyazic acid, is formed.

A sketch of Gay-Lussac's researches respecting prussic acid has been inserted in the last number of the *Annals of Philosophy*. He has shown that it is a compound of equal volumes of cyanogen and hydrogen gas. Hence it consists of

Carbon	45·10	2 atoms
Azote	52·72	1
Hydrogen	2·18	1
	100·00	

It may be obtained by pouring muriatic acid upon *cyanide of mercury*. The receiver must be surrounded with ice. Gay-Lussac has given it the name of *hydro-cyanic acid*. It possesses the following properties:—

It is a colourless liquid, having a very strong smell. Its taste is at first cooling, then hot, and it is a violent poison. Its specific gravity is 0·6969. It boils at the temperature of 80°, and congeals at the temperature of 5°. When exposed to the air, it begins to evaporate, and produces a degree of cold sufficient to congeal it. The specific gravity of its vapour is 0·9360. This acid combines with the different bases, and forms the salts at present known by the name of *prussiates*. Henceforth they must be called *hydro-cyanates*. Potassium, and potash, soda, and barytes, at a red heat, decompose hydro-cyanic acid, and produce cyanides.

5. The substance discovered by Berthollet, and called by him

oxyprussic acid, formed by mixing chlorine and prussic acid, has likewise been examined by Gay-Lussac. He has shown it to be a compound formed by the union of equal volumes of chlorine gas and cyanogen gas, and in consequence has given it the name of *chloro-cyanic acid.* This acid is a colourless liquid, having a very strong smell, and acting on the eyes and nose nearly as strongly as ammonia. It reddens litmus; is not combustible. The specific gravity of its vapour is 2·111. Its solution in water does not precipitate nitrate of silver nor barytes-water. Alkaline bodies seem to combine with it; but the instant an acid is poured on the compound, the chloro-cyanic acid is decomposed, and carbonic acid and ammonia formed. Gay-Lussac has rendered it probable that in this case the whole chloro-cyanic acid and the portion of water decomposed are resolved into

1 volume muriatic acid,
1 volume carbonic acid,
1 volume ammoniacal gas.

6. In the historical introduction published in the *Annals of Philosophy* at the beginning of last year (vol. v. p. 25), I gave an account of ferrureted chyazic acid and sulphureted chyazic acid, two new compounds discovered by Mr. Porrett. Since that time this ingenious chemist has subjected these acids to a new analysis. The results which he obtained are as follows. Ferrureted chyazic acid is composed of

Prussic acid	63·79	...... 4 atoms
Black oxide of iron ...	36·21	 1
	100·00	

Sulphureted chyazic acid is composed of

Prussic acid	34·8	...... 1 atom
Sulphur	65·2	 4
	100·0	

I should not be surprised if these two acids were in reality compounds of cyanogen with iron and sulphur; that is to say, *cyanides of iron and sulphur*. As some of Mr. Porrett's data were erroneous, it is obvious that his conclusions cannot be quite correct.

7. *Chromic Acid.*—In the year 1812 an elaborate set of experiments on chromic acid by Mr. Brandenburg, an apothecary at Polotzk, in Russia, was read before the Imperial Academy of Sciences at St. Petersburgh. It was published in the Russian language in the technological Journal of the Academy, vol. x. 1813. The original German copy is inserted in Schweigger's Journal (vol. xiii. p. 274). The object of the experiments is to show that the substance called *chromic acid* by Vauquelin is not a simple combination of chromium and oxygen; but that it always contains a portion of the acid employed to separate it from the alkaline body with which it was combined, and that to it only it owes its acid

properties. In short, Brandenburg seems to be of opinion that no such substance as chromic acid exists. As the subject is of considerable importance in a chemical point of view, I shall insert either the whole of Brandenburg's paper, or at least a full abstract of it, in a future number of the *Annals of Philosophy*.

8. *Oxalic Acid.*—It is well known, I presume, to most of my readers, that Mr. Royston published some time ago in the Medical Repository, the case of a young lady who died in 40 minutes in consequence of swallowing half an ounce of crystals of oxalic acid instead of sulphate of magnesia. M. Guyton-Morveau, who has given an account of this remarkable fact in the Ann. de Chim. (vol. xciii. p. 199), seems to entertain some doubts whether the death of the young lady was owing to the oxalic acid or not. Perhaps I do not understand his meaning; but he terminates his observations with the following remark: "It is difficult to avoid believing that in the administration of the remedy there took place one of those mistakes which unfortunately are too common, and that every thing done afterwards had less for its object to discover the truth than to destroy suspicion."

Whatever Morveau may mean by this paragraph, there can be no doubt that oxalic acid acts as a violent poison when taken internally; for Mr. Anthony Tod Thomson, surgeon in Sloane-street, and one of the editors of the London Medical Repository, gave it to dogs and other animals, and it never failed to prove fatal in a short time.

9. *Sorbic Acid.*—This is an acid lately discovered by Mr. Donovan in the berries of the *pyrus aucuparia*. He obtained it by the following process. The berries are bruised, and their juice squeezed out. This juice, being strained, is mixed with a filtered solution of acetate of lead. The precipitate is collected on a filter, and edulcorated with cold water. Then a very large quantity of boiling water is poured on the filter, and collected in glass jars. After some hours, this liquid deposits crystals of singular lustre and beauty. These crystals are collected, and boiled with a quantity of very dilute sulphuric acid not sufficient to saturate all the lead. The liquid is then set aside for some days. The sulphate of lead deposited being separated, a current of sulphureted hydrogen gas is passed through the liquid. The sulphuret of lead formed is separated, and the liquid boiled till the excess of sulphureted hydrogen is driven off. The water now contains nothing but sorbic acid.

This acid is colourless, without smell, and having an intensely acid taste. It dissolves in alcohol, and is very soluble in water. When evaporated to dryness, it leaves an uncrystallizable residuum, which deliquesces. When distilled, the liquor that passes over is not acid. It may be kept for a considerable length of time without undergoing decomposition. It decomposes malate of lead. It combines in three proportions with oxide of lead, forming *supersorbate*, which can only be obtained in a liquid state; *sorbate*, which constitutes the beautiful silky crystals from which the acid was ob-

tained; and *subsorbate*, which is the insoluble hard residue left upon the filter. Both of the last two salts are insoluble in water; but when the sorbate is boiled in water, it is decomposed into supersorbate and subsorbate. The supersorbate remains dissolved in the liquid; and, on cooling, deposits crystals of sorbate, while the excess of acid remains in the liquid. Malic acid only combines with oxide of lead in two proportions, forming supermalate and malate of lead.

Sorbic acid in excess forms with potash, soda, and ammonia, salts which yield permanent crystals. The salts of malic acid with the same bases do not crystallize.

Sorbic acid is neutralized by carbonates of lime and barytes, while these carbonates are incapable of neutralizing malic acid.

Sorbate of magnesia crystallizes; malate of magnesia yields no crystals.

Sorbic acid does not dissolve alumina.

VII. SALTS.

1. *New Triple Salt.*—Mr. Geiger, apothecary at Karlsruhe, evaporated the liquid that remained after the preparation of muriatic acid from a mixture of sulphuric acid and common salt, from which he had separated all the crystals of sulphate of soda that he could obtain. He procured a salt which possessed the following properties:—

It crystallized in transparent rectangular oblong tables or square tables, from the size of $\frac{1}{4}$ inch to $\frac{1}{2}$ inch and $\frac{1}{3}$ of a line in thickness. Sometimes small crystals appeared nearly of the cubic form. Its taste was cooling, and similar to that of sulphate of soda. It did not effloresce. At the temperature of 68° it dissolves in twice its weight of water. When the solution is cooled, crystals of common sulphate of soda are deposited. He found by analysis that this salt was composed of

Sulphuric acid	23·800
Muriatic acid	0·176
Soda	18·524
Water of crystallization	57·500
	100·000

I cannot, for my part, consider this as a true triple salt. The proportion of muriatic acid is too small. Besides, the proportions of sulphuric acid and soda are just those which exist in sulphate of soda; namely, two atoms of sulphuric acid and one atom of soda. The muriatic acid seems only to be mechanically mixed. Probably by its attraction for water it prevents the efflorescence of the salt. The shape of the crystals is not so easily accounted for.

2. *Crystallized Ammonio-Muriate of Rhodium.*—For Dr. Wollaston's method of separating palladium and rhodium held together in solution in muriatic acid Vauquelin has substituted the following. Into the solution of the two metals, which must contain an excess

of acid, he pours a quantity of ammonia. The palladium is immediately separated in the state of a triple salt. The residual liquid being evaporated to dryness, and digested in alcohol, all the foreign salts are removed, and pure ammonio-muriate of rhodium remains. Laugier obtained this salt in regular crystals by the following method. He dissolved it in a very little water. A yellowish matter consisting of foreign bodies remained undissolved. The liquid was again evaporated to dryness, and the dry mass digested in alcohol. These solutions, evaporations, and digestions, in alcohol, being repeated a number of times, the liquid left to itself deposits regular crystals of ammonio-muriate. These crystals appear nearly black; but by transmitted light they exhibit a garnet-red colour. They have the shape of equilateral four-sided prisms. When reduced to powder, they assume a beautiful red colour.

3. *Chlorates.*—M. Vauquelin has published a description of the chlorates (Ann. de Chim. vol. xcv. p. 96). Except the chlorate of potash, which has been long known, none of these bodies had been examined by any other chemist than Mr. Chenevix, who published a description of them a good many years ago in the Philosophical Transactions.

(1.) *Chlorate of Strontian.*—It may be formed by saturating chloric acid with carbonate of strontian. It has a sharp and somewhat astringent taste. It is difficult to obtain it in crystals, because it is very soluble in water, and even deliquescent. On red-hot charcoal it melts, giving out a fine purple flame.

(2) *Chlorate of Ammonia.*—It may be obtained by saturating chloric acid with carbonate of ammonia. It crystallizes in fine needles. It seems to be volatile. Its taste is extremely sharp. It detonates on a hot body, like nitrate of ammonia, but at a lower temperature, and it gives out a red flame. When heated in close vessels, it is decomposed, and converted into chlorine gas, azotic gas, with a little oxygen gas, or oxide of azote. At the same time a little muriate of ammonia is formed.

(3.) *Chlorate of Soda.*—It may be obtained by saturating chloric acid with carbonate of soda. It crystallizes in square plates, like the chlorate of potash; but it is very soluble in water, yet not deliquescent. Its taste is cooling, and somewhat sharp. On red-hot charcoal it fuses into globules, giving out a yellow light.

500 parts of carbonate of soda saturated with chloric acid produced 1100 of crystallized chlorate. When distilled, it yielded a great deal of oxygen gas mixed with a little chlorine. The residuum of the distillation was distinctly alkaline, though it had not been strongly heated.

(4) *Chlorate of Barytes.*—It crystallizes in rectangular four-sided prisms terminated by an oblique face. Its taste is sharp and harsh. It dissolves at the temperature of 50° in about four times its weight of water. It is insoluble in alcohol. Its aqueous solution, when pure, is neither precipitated by nitrate of silver nor muriatic acid.

When well dried and heated, it loses 39 per cent., which loss is owing to the escape of oxygen. The residue of this decomposition

is not entirely soluble in water, and the solution is sensibly alkaline. The insoluble portion is carbonate of barytes.

Vauquelin endeavoured from this salt to determine the composition of chloric acid, and the proportion of oxygen in barytes; but his experiments varied so much from each other, that no confidence can be placed in them. He found chloric acid composed of

Oxygen	65
Chlorine	35
	100

and barytes of 100 barium + 7 oxygen.

5. *Prochlorate of Mercury.*—This salt is obtained by dissolving protoxide of mercury in chloric acid. When the saturation is complete, almost the whole salt precipitates in the form of small grains. Its colour is greenish yellow. Its taste is mercurial, but weak. It is slightly soluble in boiling water. When heated, it detonates, and oxygen gas is given out; but it is chiefly converted into corrosive sublimate and peroxide of mercury; the chlorine combining with one portion of the mercury, and the oxygen with another.

(6.) *Perchlorate of Mercury.*—This salt may be obtained by dissolving peroxide of mercury in chloric acid. It is pretty soluble in water, having a strong taste analogous to that of corrosive sublimate. It always contains an excess of acid, is precipitated yellow by the alkalies, and crystallizes in small needles. When heated, it gives out oxygen, and is converted into corrosive sublimate and oxide of mercury. If the heat be increased, more oxygen is driven off, and most of the corrosive sublimate is converted into calomel.

4. I shall here collect Berzelius's analyses of several salts as he has stated them in his paper on the Composition of Vegetable Substances, published in the fifth volume of the *Annals of Philosophy*.

Citrate of Lead.

Citric acid	34·18	100
Oxide of lead	65·82	190
	100·00	

Tartrate of Lead.

Tartaric acid	37·5	100
Oxide of lead	62·5	167
	100·00	

Tartrate of Lime.

Tartaric acid	50·55
Lime	21·64
Water	27·81
	100·00

Oxalate of Lead.

Oxalic acid	24·54	100
Oxide of lead	75·46	307·5
	100·00	

Succinate of Lead.

Succinic acid	30·9	100
Oxide of lead	69·1	223·62
	100·0	

Subsuccinate of Lead.

Succinic acid	13·07	100
Oxide of lead	86·93	666
	100·00	

Acetate of Lime.

Acetic acid	64·6	100
Lime	35·4	54·8
	100·0	

Acetate of Lead.

Acetic acid	26·97	31·48	100·000
Oxide of lead	58·71	68·52	217·662
Water	14·32		53·140
	100·00	100·00	

Subacetate of Lead.

Acetic acid	13·23	100
Oxide of lead	86·77	656
	100·00	

Gallate of Lead.

Gallic acid	36·5	100
Oxide of lead	63·5	173·97
	100·0	

Subgallate of lead.

Gallic acid	15·92	100
Oxide of lead	84·08	528
	100·00	

Saclactate of Lead.

Saclactic acid	48·33	100
Oxide of lead	51·66	106·87
	99·99	

Benzoate of Lead.

Benzoic acid	51·65	49·56	100
Oxide of lead	48·35	46·49	93·61
Water		3·85	
	100·00	100·00	

Sub-benzoate of Lead.

Benzoic acid	26	100
Oxide of lead	74	284
	100	

Tannate of lead.

Tannin	65·79	100
Oxide of lead	34·21	52
	100·00	

Saccharate of Lead.

Sugar	41·74	100
Oxide of lead	58·26	139·6
	100·00	

Saccharate of Ammonia.

Sugar	90·00	100
Ammonia	4·93	5·49
Water	5·07	5·60
	100·00	

Saccolate of Lead.

Sugar of milk	36·471	100
Oxide of lead	63·529	174·15
	100·000	

Supersaccolate of Lead.

Sugar of milk	81·877	100
Oxide of lead.	18·123	22·1
	100·000	

Subsaccolate of Lead.

Sugar of milk	12·8	100
Oxide of lead	87·2	681
	100·0	

Gummate of Lead.

Gum-arabic.	61·75	100
Oxide of lead	38·25	62·105
	100·00	

Amylate of Lead.

Potatoe starch	72	100
Oxide of lead	28	38·89
	100	

5. *Action of Sugar on Metalline Salts.*—Vogel has published a long paper to show that when sugar is boiled with various metallic oxides and with different metalline salts, it has the property of decomposing them. Sometimes it reduces the oxide to the metallic state; at others (and this most frequently) it deprives the oxide of one of the doses of oxygen with which it was combined, and thereby reduces it to an inferior degree of oxidation. The result of his experiments is as follows:—

When a solution of acetate of copper is boiled with sugar, no gas is evolved; but a brown powder is precipitated, which is protoxide of copper. Sugar of milk, honey, manna, and other sweet bodies, produce the same effect. Scheele's sweet principle of the oils, fat, and wax, likewise occasion the same precipitation, but much more slowly.

When sulphate of copper and sugar are boiled together, the copper is precipitated in the metallic state. All the other sweet substances produce the same effect.

When nitrate or muriate of copper is boiled with sugar, no protoxide precipitates; but the salts are converted into *pronitrates* and *promuriates*. The salts of iron, zinc, tin, and manganese; in short, of all the metals which have the property of decomposing water, are not decomposed by sugar.

Sugar boiled with nitrate of mercury throws down metallic mercury. It produces no effect upon calomel; but converts corrosive sublimate into calomel.

Nitrate of silver and muriate of gold are very readily decomposed by sugar. Sugar and manna convert peroxide of mercury into protoxide.

Sugar readily dissolves the red oxide of lead or litharge. It deprives the brown oxide of lead of part of its oxygen, and then dissolves it.

VIII. MINERAL WATERS.

1. *Sea Water.*—Various analyses of sea water have been lately published by Lichtenberg, Vogel, and Bouillon Lagrange. Mr. Pfaff, of Kiel, one of the most accurate of the German chemists, was induced by these experiments to make a careful analysis of the waters of the Baltic, which wash the coast of Germany at Kiel. The result of his analysis is as follows.

The specific gravity of the water was 1·014. 100 grains of it contained the following salts:—

Carbonate of magnesia	0·25
Muriate of magnesia	1·95
Muriate of lime	0·07
Sulphate of lime	0·34
Sulphate of magnesia	2·00
Common salt	13·08
	17·69

He shows that muriate of lime and sulphate of magnesia are not incompatible salts, as has been generally supposed; but that they may exist together in solutions sufficiently diluted with water. His dissertation was published in September, 1814 (Schweigger's Journal, vol. xi. p. 8); so that he anticipated Dr. Murray, of Edinburgh, who has advanced an analogous opinion in his analysis of the mineral waters of Dunblane and Pitcaithly.

2. *Geilenauer Mineral Water.*—This water was likewise analyzed by Pfaff. He conceives that during the carriage a portion of the carbonic acid which it originally contained was lost. The iron, which constitutes an ingredient of this water at the spring, had precipitated in the form of a brown powder. In a civil pound of this water he found the following ingredients:—

Carbonate of lime	4·8 grs.	Common salt	4·0 grs.
Carbonate of soda	4·0	Carbonic acid gas	26 Paris cub. in.

3. *Mineral Water of Dunblane.*—This newly discovered mineral water has been lately analyzed by Dr. Murray, of Edinburgh, whose valuable paper on the subject has been published in the fourth and fifth numbers of the last volume of the *Annals of Philosophy.* The specific gravity of the water was 1·00475. A pint of it was found to contain the following salts:—

Common salt	24
Muriate of lime	18
Sulphate of lime	3·5
Carbonate of lime	0·5
Oxide of iron	0·17
	46·17

But he supposes that the sulphate of lime does not exist in the water originally, but is formed during the evaporation by the action of sulphate of soda on muriate of lime. According to this very probable idea, the real constituents of this water are as follows:—

Common salt	21
Muriate of lime	20·8
Sulphate of soda	3·7
Carbonate of lime	0·5
Oxide of iron	0·17
	46·17

4. *Mineral Water of Pitcaithly.*—This water has been long known and frequented by the inhabitants of Scotland. Dr. Murray has likewise subjected this water to an analysis. He found the saline contents in a wine pint to be

Common salt	13·4
Muriate of lime	19·5
Sulphate of lime	0·9
Carbonate of lime	0·5
	34·3

And the gaseous contents, as ascertained by Messrs. Stoddart and Mitchell, are

Atmospheric air	0·5 cub. in.
Carbonic acid gas	1·0

But Dr. Murray conceives that the sulphate of lime is produced during the analysis by the action of sulphate of soda on muriate of lime. According to this opinion, the real constituents of this mineral water are

Common salt	12·7
Muriate of lime	20·2
Sulphate of soda	0·9
Carbonate of lime	0·5
	34·3

M. Vogel, of Paris, has likewise started a similar opinion with Dr. Murray, namely, that muriate of lime and sulphate of magnesia may exist in the same liquid; but as his paper on the subject was only published in July, 1815, (Schweigger's Journal, vol. xiii. p. 344,) he did not anticipate the British chemist. Pfaff, however, must be admitted to have anticipated him by at least a year. The ingenious opinion, however, the accuracy of which can scarcely be called in question, that the salts contained in mineral waters are often decomposed and altered during the analysis, belongs, as far as I know, originally to Dr. Murray.

IX. VEGETABLE BODIES.

1. The most important paper which has appeared upon vegetable substances is by Professor Berzelius, and was published in the fifth volume of the *Annals of Philosophy*. It describes a set of very elaborate and successful experiments to determine the composition of the vegetable acids and several other vegetable bodies. Gay-Lussac and Thenard had previously published a set of very ingenious experiments upon the same subject; but as they were not at sufficient pains to dry the substances which they analyzed, it is not easy to draw consequences from their experiments. Berzelius has shown that their rules respecting the nature of vegetable substances depending on the relative proportions of oxygen and hydrogen which

they contain do not hold good. The following are the results which Berzelius obtained:—

Substances.	Composition. Per Cent. Oxygen.	Carbon.	Hydrogen.	In Atoms. Oxy.	Carbon.	Hydrogen.
Citric acid	54·831	41·369	3·800	1	1	1
Tartaric acid	60·213	35·980	3·807	5	4	5
Oxalic acid	66·534	33·222	0·244	18	12	1
Succinic acid	47·888	47·600	4·512	3	4	4
Acetic acid	46·82	46·83	6·35	3	4	6
Gallic acid...........	38 36	56·64	5·00	3	6	6
Saclactic acid	61·465	33·430	5·105	8	6	10
Benzoic acid	20·43	74·41	5·16	1	5	3
Tannin (from nutgalls)	44·654	51·160	4·186	4	6	6
Common sugar........	51·47	41·48	7·05	10	12	21
Sugar of milk	53·359	39·474	7·167	1	1	2
Gum-arabic	51·306	41·906	6·788	12	13	24
Potatoe starch........	49·455	43·481	7·064	6	7	13

I conceive that I have given satisfactory proofs (*Annals of Philosophy*, vol. v. p. 187), that oxalic acid contains more hydrogen than Berzelius obtained in his analysis, and that it is in reality composed of

Oxygen .. 3 atoms | Carbon .. 2 atoms | Hydrogen .. 1 atom

I think it may be easily proved in the same way that the true composition of the other vegetable acids analyzed by Berzelius is as follows:—

Substances.	Composition. Per Cent. Oxygen.	Carbon.	Hydrogen.	In Atoms. Oxy.	Carbon.	Hydrogen.
Citric acid	55·036	41·332	3·632	2	2	1
Tartaric acid	59·524	35·762	4·714	5	4	3
Oxalic acid	64·739	32·413	2·848	3	2	1
Succinic acid	47·923	47·859	4·218	3	4	2
Acetic acid	46·875	46·938	6·187	3	4	3
Gallic acid	38·098	56·892	5·010	1	2	1
Saclactic acid	60·763	34·225	5 012	8	6	5

As for Benzoic acid I suspect Berzelius has committed some mistake in the analysis of it; for its composition as derived from the benzoate of lead does not agree in the least with the statement of this most ingenious chemist. I do not at present notice the analysis of the five last substances in Berzelius's table, because the subject is attended with certain difficulties which would require more room to explain them than I can afford at present.

Saussure has given us the analysis of several vegetable substances.

His experiments were made with great care; but his method is perhaps scarcely susceptible of the precision which is requisite in such delicate investigations. His numbers for gum arabic differ very much from those of Berzelius. The following are the results which he obtained. I have added the nearest atoms, neglecting the azote; because I am not quite satisfied respecting its presence in the substances subjected to analysis.

	Composition.						
	Per Cent.				In Atoms.		
Substances.	Oxy.	Carbon.	Hydr.	A zot.	Oxy.	Carbon.	Hyd.
Starch of wheat	48·31	45·39	5·90	0·4	4	5	1
Starch Sugar	55·87	37·29	6·84	—	8	9	1
Sugar of Grapes	56·51	36·71	6·78	—	7	8	1
Manna	45·80	47·82	6·06	0·32	8	13	1
Gum-arabic	48·26	45·84	5·46	0·44	4	5	1

2. *Conversion of Starch into Sugar.*—The curious fact, first ascertained by Kirchoff, that starch when boiled in very diluted sulphuric acid is converted into sugar, has lately engaged a good deal of the attention of chemists. Fourcroy takes notice that when starch is treated with muriatic acid or chlorine it acquires a sweet taste. (General System of Chemical Knowledge, vol. viii. p. 157. English Trans.) Einhoff, in his elaborate analysis of the potatoe (Gehlen's Journal, vol. iv. p. 445) showed that the mucilaginous matter of that root could be converted into a saccharine matter. His process was complicated; but one part of it consisted in digesting the substance with acetic acid. This probably produced the effect. Nasse has shown that the starch extracted from raw potatoes is easily converted into sugar; but that if the potatoe be boiled, or subjected to fermentation, the starch obtained from it is not convertible into saccharine matter: hence he concludes, that starch only from living vegetable matter is susceptible of this change, while the starch extracted from dead vegetable matter is incapable of it. But this conclusion seems a little too general. When potatoes are exposed to frost they become soft and sweet, and completely lose the property of vegetating; they are, therefore, reduced to dead vegetable matter. But the starch extracted from them in this state is perfectly similar to that from fresh potatoes. I have no doubt, therefore, that it might be converted into sugar by the usual process, though I have never had an opportunity of making the experiment. Nasse has shown that the opinions entertained by Fourcroy respecting the saccharine fermentation are erroneous, and that during the conversion of mucilaginous matter into sugar no fermentation whatever takes place.

Vogel of Paris has shown that when starch is converted into sugar by boiling it in diluted sulphuric acid, no gas whatever is

extricated; but the most curious and complete set of experiments on this subject are those of De Saussure, inserted in the last number of the *Annals of Philosophy*. He has shown that no gaseous products are exhaled; that the quantity of sulphuric acid is not altered; and that the weight of the sugar obtained is greater than that of the starch from which it was produced: hence he concludes that the sugar is merely a combination of the starch with water, and that the only use of the acid is to produce a solution of the starch, in which state only it is capable of combining with water.

Nasse has pointed out the differences between starch sugar and common sugar from the sugar cane. Starch sugar assumes the form of spherical crystals like honey. It is not so hard as common sugar. It is not so soluble in water. Its sweetening power, according to the experiments of Kirchhoff, is to that of common sugar as 1 to $2\frac{1}{2}$. When digested with an alkaline carbonate a mucilaginous matter precipitates. This precipitate is obtained in greater abundance when the solution of starch sugar is mixed with muriate of tin. When dissolved in water it ferments of itself, without the addition of any yeast, which is not the case with common sugar. (Schweigger's Journal, vol. x. p. 305.)

3. *Extractive.*—A very long paper has been recently published by Theodore Von Grotthuss (Schweigger's Journal, vol. xiii. p. 117) containing experiments chemical and galvanic upon a great number of vegetable substances. It is not possible to give an abstract of it without taking up a great deal more room than I can at present spare; but the object of the paper seems to be to show that the vegetable substance called *saponaceous matter* (*seifenstoff*) by the Germans, is not the same with the *extractive*, as has been endeavoured to be proved by Fourcroy and by Schraader. As nobody has hitherto succeeded in obtaining either of these substances in a pure state, such discussions do not seem susceptible of much precision. Grotthuss gives the following process for obtaining saponaceous matter in a state of purity. Boil together *saponaria officinalis* and quicklime in a sufficient quantity of water. Filter the liquid. Precipitate the lime by phosphoric acid. Filter and evaporate the liquid slowly to dryness. The residuum is pure saponaceous matter.

4. *Cinchona.*—For the first set of experiments on the different kinds of Peruvian bark we are indebted to Vauquelin, who was, I believe, the first person who distinguished one of the constituents of that substance by the name of *cinchonin*. A Portuguese of the name of Gomes published a new set of experiments on this subject in the Edinburgh Medical and Surgical Journal (1811) and announced the discovery of a new species of cinchonin. To verify this discovery, a set of experiments was made by Dr. Van Smissien, in Professor Pfaff's laboratory at Keil, under the direction and by the assistance of the Professor. These constituted the subject of

his inaugural dissertation published at Keil in 1813. The following is an abstract of the results obtained.

Sixteen ounces of the best bark were digested for three days in 48 ounces of alcohol, of the specific gravity 0·819, being often agitated during that time. The alcohol was then poured off and 48 additional ounces poured on the bark, and allowed to remain for two days. The bark powder, by this treatment, was so exhausted of soluble matter, that four pounds of water only formed with it an opal-coloured tasteless solution. The alcoholic tincture was distilled in a retort almost to the consistence of a syrup, and then mixed with 36 ounces of distilled water. There precipitated a light brown powder, which, when well washed on the filter, became white; but assumed a darker colour on drying. It weighed a half ounce and 40 grains.

The filtered aqueous solution had a dark reddish brown colour, a very bitter and astringent taste; but was not sour, though it reddened litmus paper. It was mixed with a solution of pure carbonate of potash. A precipitate of a light rose-red colour fell down. It weighed two drachms and 45 grains. The liquid, which had assumed a darker colour, was saturated with sulphuric acid. This occasioned the separation of a very bulky reddish-brown flocky precipitate, which only weighed 18 grains. It was insoluble in alcohol, but dissolved readily in water. This solution, when mixed with sulphate of iron, became olive-green, and let fall a trifling precipitate. It was precipitated likewise by infusion of nutgalls and tartar emetic; but not by solution of isinglass.

Of these precipitates, the first, obtained by mixing the distilled alcoholic tincture with water, is the substance which Gomes considered as a new species of cinchonin. The following experiments were made upon it. Three drachms and 40 grains were dissolved in alcohol of 0·820 specific gravity; the solution was allowed to evaporate very slowly. Part of the substance fell down in the state of a reddish-brown precipitate. Another portion formed thin coats upon the sides of the vessel. These were transparent, and when light was viewed through them, assumed the appearance of a collection of needle-form crystals. These coats possessed the following properties. 1. They dissolved readily in alcohol. 2. Boiling water dissolved about one sixth of its weight of them. 3. Caustic alkali readily dissolved them, and they were precipitated unaltered by the addition of sulphuric acid. 4. They were dissolved by concentrated sulphuric acid, and precipitated of a darker colour by carbonate of potash. 5. They were insoluble in sulphuric ether. 6. When put upon red-hot charcoal, they gave out an aromatic odour, and burnt with a light-coloured flame. 7. The tincture of nutgalls was not in the least altered by their solution in alcohol. They scarcely altered the solution of isinglass; but sulphate of iron gave them a strong green colour, and occasioned a precipitate. The muriate of tin occasioned no change. Chlorine thrown into

their solution occasioned the precipitation of lemon-yellow flocks. From these properties Pfaff considers the substances in question as a peculiar species of resin of cinchona.

At the same time a set of experiments was made to determine whether infusion of nutgalls, tartar emetic, and gelatin were precipitated by one and the same constituent in Peruvian bark, or by different constituents. The following were the results obtained.

The substance which at once precipitates tartar emetic, infusion of nutgalls, and gelatin, is equally soluble in water and alcohol, and possesses the properties of those vegetable bodies which have received the name of *saponaceous matters*.

The substance which precipitates infusion of nutgalls and tartar emetic appears to exist in all the varieties of cinchona; but to vary in its properties in the different species.

The substance which precipitates the infusion of nutgalls is that in which the bitter taste of the Peruvian bark resides; yet its combination with infusion of nutgalls has no bitter taste.

The substance which precipitates gelatin differs entirely from the last mentioned constituent. It belongs to that modification of tannin which precipitates iron of a green colour.

5. *Pollen of Tulips.*—Professor John, who is one of the most active and laborious chemists in Germany, and has already published a number of volumes of analyses, chiefly of vegetable and animal substances, has, among other things, turned his attention to the pollen of vegetables. The pollen, he finds, always contains a peculiar substance, which has hitherto been considered as *albumen*; but to which he has given the name of *pollenin*. This substance forms with nitric acid a bitter tasted matter. It is insoluble in alcohol, ether, water, oil of turpentine, naphtha, carbonated and caustic alkalies; and when distilled yields ammonia and an acid liquid. The pollenin of different plants varies somewhat in its properties.

John has also found that wax, whether extracted from vegetables or bees'-wax, consists of two constituents: the first of which, which is soluble in alcohol, he calls *cerin*; the second, which is insoluble in that liquid, he calls *myricin*. The following is his analysis of the pollen of tulips.

He digested the pollen in a sufficient quantity of alcohol to take up every thing soluble. There remained a bluish-green powder. This was pollenin, coloured by a blue pigment which exists in this pollen; for the natural colour of pollenin is yellow.

The alcohol solution, when filtered, had a violet-blue colour, and gradually let fall a precipitate, which was *cerin*. The liquid freed from cerin, being evaporated, let fall a violet-blue extractive matter, which was soluble both in water and alcohol. The aqueous solution of this substance possessed the following properties.

It precipitated sugar of lead emerald green.
lime water, ditto.
muriate of barytes, no change.
nitrate of mercury, violet blue.

Acids rendered the solution red, nitrate of silver rendered it carmine red.

When this substance was dried and dissolved in lime-water, and separated by evaporation, malate of lime remained in solution. When the lime was removed by means of sulphuric acid, and the malate of lime by means of alcohol, a sweet-tasted substance remained, which would not crystallize.

The pollen, when burnt, left an ash, which contained potash, magnesia, and lime. Thus the constituents of the pollen of tulips were as follows:

Pollenin.
A saccharine not crystallizable matter.
A very little cerin.
A violet-blue pigment soluble in water and alcohol.
Malates of potash, lime, and magnesia, with an excess of acid.
A trace of other salts with the same bases.
Caseous albumen.

Theodore Von Grotthus has likewise published an analysis of the pollen of tulips (Schweigger's Journal, vol. xi. p. 281). He does not seem to have been aware of what had been previously done by John, and did not obtain all the constituents mentioned by that chemist. According to the analysis of Grotthus, 26 grains of pollen of tulips are composed of the following ingredients:

Fibrous vegetable albumen	9 grs.
Dried vegetable albumen	7
Soluble vegetable albumen	4¼
Malate of lime with some malate of magnesia	3½
Malic acid	1
Malate of ammonia, Nitre?, Fibrin	1¼
	26

The two first substances in this table constitute in all probability the pollenin of John.

6. *Alcornoque.*—A new medicinal substance has been lately brought to Germany from Martinique. It is the root of an unknown plant to which the Indians have given the name of alcornoque. Dr. Rein of Leipzig has subjected it to a chemical analysis. He found it composed of the following constituents. (Gilbert's Annalen der Physik, vol. l. p. 121)

Gum	0·105
Saponaceous matter	0·102
Resin	0·054
Volatile matter	0·136
Fibrin	0·603
Trace of tartaric acid	
	1·000

7. *Sap of the Vine.*—Dr. Prout has made a chemical analysis of this sap. (*Annals of Philosophy*, vol. v. p. 109.) It was slightly whitish like river water, had a sweetish taste, and its specific gravity did not sensibly differ from that of pure water. Alkalies reddened it, and threw down a flocky precipitate, which was redissolved by acetic acid. Oxalate of ammonia threw down a white precipitate. 460 grains of it being evaporated left only $\frac{1}{5}$th of a grain of residuum. One half of this was carbonate of lime; the rest was a peculiar vegetable matter, not soluble in alcohol. It gave traces also of carbonic acid, acetic acid, and of an alkali which was probably potash.

8. The juice of the ribes grossularia, or green gooseberry, yielded to Dr. John the following constituents:

Much water.
Uncrystallizable sugar.
Supercitrate of potash.
Supermalate of potash.
A little resin.
Prunin or cerasin.
Insoluble modified gum.
A salt with base of magnesia.
Traces of phosphates of lime and magnesia.
Trace of muriate of lime?
A little phosphate? of iron.
Ammonia, probably combined with citric and malic acids.
Fibrin.

9. 300 parts of angelica Archangelica dried yielded the same chemist the following constituents:

Colourless and very volatile oil	
Gum	100·5
Inulin	12
Bitter extractive	37·5
Sharp tasted resin	20
A peculiar substance soluble only in potash	22
Woody fibres	90
Water and loss	18
	300

The earthy constituents were

Phosphate of lime.
Phosphate of iron.
Phosphate of magnesia.
Silica?

10. The juice of the Leontodon taraxicum, or dandelion, yielded the same chemist the following bodies:

Water.
Caoutchouc.
Bitter extractive.
A sweet substance?
A trace of resin.
A trace of gum?
An acid.
Muriate, phosphate, and sulphate of lime, and of an alkali.

11. The milky juice of the ficus carica, or fig-tree, yielded him

Caoutchouc.
Resin, soluble only in boiling alcohol.
A trace of extractive soluble in water.
Salts.

12. The milky juice yielded by the young bark and wood of the

platinus occidentalis, or plane-tree, gave to the same chemist the following constituents:

Water.
Resin, soluble only in boiling alcohol.
Caoutchouc.
A very small quantity of gummy matter.
Phosphoric acid.
Salts.

13. M. Gaultier de Claubry has lately analyzed several species of fuci. (Ann. de Chimie, vol. xciii. p. 75, 113.) The following are the results which he obtained.

Fucus saccharinus yielded the following constituents:

A peculiar saccharine matter.
Mucilage.
Albumen.
Green colouring matter.
Oxalic acid } both, probably,
Malic acid } united to potash.
Sulphate of potash.
———— soda.
———— magnesia.
Muriate of potash.
Common salt.
Muriate of magnesia.
Sulphureted sulphite of soda.
Subcarbonate of potash.
———— soda.
Hydriodate of potash.
Silica.
Subphosphate of lime.
———— magnesia.
Oxide of iron, probably combined with phosphoric acid.
Oxalate of lime.

The constituents of the *fucus digitatus* were similar; but the quantity of iodine which it contained was smaller.

The *fucus vesiculosus* contained a vegeto-animal matter which separated from the aqueous decoction during the evaporation, and which appeared to give the disagreeable taste and odour by which that liquid is distinguished. It contained likewise a vegetable matter, soluble in water and alcohol, of a sweetish taste, which became at last bitter: and a vegetable matter, soluble in alcohol, which precipitated during the evaporation in a reddish-green powder. Finally, it contained the same salts as the fucus saccharinus, though in very different proportions. It contained very little iodine.

Professor John published an analysis of this fucus from the Baltic Sea, in the month of August last, (Schweigger's Journal, vol. xiii. p. 464). He obtained from 100 parts of the dried plant the following substances:

A brownish red mucilaginous matter Flesh-red extractive, with some sulphate and muriate of soda. A peculiar acid.	4
Resinous fatty matter	2
Sulphate of soda with some common salt	3·13
Sulphate of lime with much sulphate of magnesia and some phosphate of lime Some oxides of magnesia and iron.	12·87
A membranous matter, which John calls fucous albumen Silica ?	78
	100

John could not succeed in detecting any iodine in this fucus; but his experiments were made upon a small quantity of the fucus, and he did not employ any very delicate test.

Gaultier de Claubry found the *fucus serratus* to contain albumen, a mucilaginous substance precipitated from water by alcohol, and of a dark colour; green colouring matter soluble in hot alcohol and precipitating as the liquid cools;* another vegetable matter, having little taste and soluble in water and alcohol; the same salts as the fucus saccharinus, but much more subcarbonate of soda and more iodine than the *fucus vesiculosus.*

The *fucus siliquosus* was found by him to contain the following substances. A great deal of vegeto-animal matter (albumen); a brownish-red mucus; a bitter substance soluble in alcohol; a matter soluble in hot alcohol and precipitating in greenish-brown flocks on evaporating the liquid; the same salts as the fucus saccharinus, but very little iodine.

The *fucus filum* contained a scarcely sensible quantity of vegeto-animal matter; a mucous substance; a small quantity of matter which precipitates in flocks* from the alcohol that had been digested on the plant; the same salts as the fucus saccharinus, but very little iodine.

From the experiments of Gaultier de Claubry, it appears that the saccharine matter of the fuci possesses the characters of manna.

X. ANIMAL BODIES.

1. Vauquelin some time ago published an analysis of the brain of animals, from which he concluded that phosphorus was one of its constituents. This induced Professor John to undertake a set of experiments upon the brain, nerves, and spinal marrow of calves. (Schweigger's Journal, vol. x. p. 155.) These experiments induced him to conclude that phosphorus is not a constituent of the brain; but that it exists in that substance in the state of phosphate of ammonia. I must acknowledge, however, that his experiments do not appear to me quite conclusive. The subject is perhaps too delicate to be determined by chemical analysis in its present state.

He employed always brain extracted from the animal immediately after death and still warm.

The liquid portion of the brain assumed a liver colour when heated. It consisted of albumen, water, and traces of various salts. The solid matter of the brain produces no change in the colour of litmus paper, even when exposed to the air for days. John supposes that if it contained phosphorus, phosphoric acid would be formed by such exposure. When heated it gave out the odour of meat, but no fat separated. When all the liquid portion was evaporated the matter became brown, and at last melted and was charred. The silver vessel, in which the experiment was made, assumed a black colour, indicating the presence of

* Probably wax.—T.

sulphur. The water, with which the charry matter was washed, reddened litmus paper. When the water was evaporated, the residuum treated with potash yielded ammonia: this residuum, being dissolved in water, left a small portion of a substance which possessed the properties of silica. From the solution ammonia precipitated phosphate of lime. Left to spontaneous evaporation it yielded crystals of sulphate of potash, common salt, and phosphate of magnesia. The free acid present was the phosphoric.

The brain, when triturated with potash, gave out ammonia; and ammonia was likewise obtained when a mixture of brain, potash, and water was distilled.

When water is boiled with a quantity of brain, then filtered, evaporated, and mixed with alcohol, only a little gelatinous matter separates. The alcoholic solution in a few weeks deposited crystals, which consisted of a greasy matter, phosphate of ammonia, and common salt. Alcohol separates the fatty matter very well from brain, and the liquid passes readily through the filter when hot. The fatty matter of calf's brains is white. At the same time the alcohol dissolves another substance called by Thenard and Vauquelin osmazom.

The constituents of the cortical portion of the brain of a calf were as follows:

Water	75 to 80
Insoluble cerebral albumen with some soluble ditto	10
Osmazom Fatty matter Phosphate of lime Phosphate of soda Phosphate of ammonia Phosphate of magnesia A sulphate Common salt Trace of phosphate of iron	15 to 10
	100

The medullary portion of the brain contains the same constituents as the cortical; but the proportion of fatty matter is greater, and the cerebral albumen, when treated with alcohol, is harder and more fibrous. Traces of silica are also found in it.

The medulla oblongata contains the same ingredients as the medullary portion of the brain; but less water and more albumen. The same observations apply to the thalami nervorum opticorum, the cerebellum, and the nerves.

2. *Black Pigment of the Eye.*—A curious set of chemical experiments on the black pigment in the eyes of oxen and calves has been published by Leopold Gmelin (Schweigger's Journal, vol. x. p. 507). From 500 eyes of oxen and calves he collected 75 grains of this substance. Its colour is blackish-brown. It is tasteless, and adheres to the tongue like clay. It is insoluble in water,

alcohol, sulphuric ether, oils, lime-water, and distilled vinegar. It dissolves in potash and ammonia by the assistance of heat, and is again precipitated by acids. Sulphuric acid dissolves it and blackens the colour. Muriatic acid produces the same change of colour; but forms only an imperfect solution. Nitric acid dissolves it, and changes its colour to reddish-brown. $12\frac{1}{2}$ grains ($12\frac{1}{4}$ Troy grains) of the pigment were subjected to heat in a glass tube; a few drops of water came over, holding carbonate of ammonia in solution, a brown oil, and crystals of carbonate of ammonia. The gas extricated amounted to six cubic inches (6·17 cubic inches English) which consisted of

Carbonic acid gas	3
Oxygen gas	0·159
Azotic gas	2·131
Carbureted hydrogen	0·710
	6·000

The water, oil, and carbonate of ammonia that came over into the receiver weighed five grains; and the oil alone amounted to $\frac{1}{3}$ of the weight of the whole. The coal remaining in the retort weighed $5\frac{1}{2}$ grains. It consisted almost entirely of charcoal. When burnt it left $\frac{5}{12}$ths of a grain of ashes, which consisted of soda, lime, oxide of iron, and muriatic acid; and probably, also, phosphoric acid and carbonic acid. Gmelin conceives that this black pigment approaches the nature of indigo.

3. *Ink of the Cuttle Fish.*—Some experiments on this substance were published in 1813 by Mr. Grover Kemp. Dr. Prout lately analyzed a quantity of it in a dry state, which had been sent him in the original cyst in which it was contained. He found 100 parts of it to be composed as follows. (*Annals of Philosophy*, vol. v. p. 419.)

Black colouring matter	78·00
Carbonate of lime	10·40
Carbonate of magnesia	7·00
Muriate of soda? Sulphate of soda?	2·16
Mucus	0·84
Loss	1·60
	100·00

Dr. Prout did not particularly examine the colouring matter; but this has been done by Leopold Gmelin, who found it to possess very nearly the same properties with the black pigment of the eye. (Schweigger's Journal, vol. x. p. 533.) His experiments do not quite agree with those stated by Dr. Prout; probably, because they were made upon the recent and moist pigment, whereas Dr. Prout's experiments were made upon it in a dry state.

4. *Milt of the Cyprinus Tinca, the Tench.*—Fourcroy and Vauquelin published some years ago a set of experiments on the milt of

this fish, from which they concluded that it contained a quantity of phosphorus as a constituent. These experiments induced Professor John to subject the substance in question to a chemical analysis. He could not detect the presence of any phosphorus. The constituents which he found in this milt were the following: (Schweigger's Journal, vol. x. p. 168.)

Water.	Phosphate of lime.
Insoluble albumen.	Phosphate of magnesia.
Gelatine.	Alkaline phosphate.
Phosphate of ammonia.	

5. *Bile altered by Disease.*—Professor Rudolphi put into the hands of Dr. John for chemical analysis an enormous human gall bladder, which contained six ounces of altered bile, and 20 gall stones; two of which were of the size of a hazel nut. The result of the analysis of 3½ ounces of this altered bile, by Dr. John, was as follows:

	oz.	dr.	gr.
Water	3	3	39¾
Albumen	0	0	4¼
Adipocire of bile	0	0	0¼
Osmazom	0	0	9
Mucous jelly	0	0	1½
An ammoniacal salt Phosphate of lime Lime united to a combustible acid Phosphate of iron Potash, a trace Sulphate and muriate of potash Alkaline phosphate	0	0	5¼
	3½	0	0

6. *Urine in Hepatitis.*—A curious discovery, which may lead to important physiological and medical deductions, has been made by Mr. Rose of Eye, who published an account of it in the *Annals of Philosophy*, vol. v. p. 424. He found that in all diseases of the liver, whether chronic or acute, the urine contains no urea. This unexpected fact has been more recently confirmed by the experiments of Dr. Henry. (*Annals of Philosophy*, vol. vi. p. 392.) So that its truth seems to be sufficiently established.

7. *Presence of Carbonic Acid in Urine and Blood.*—Proust announced a good many years ago, that he had detected the presence of carbonates and carbonic acid in urine; but doubts were entertained of the accuracy of this opinion, because it was known that urine contains a portion of uncombined phosphoric or acetic acid. But Vogel has lately shown by a very simple experiment, that carbonic acid gas exists both in urine and blood. He put a quantity of fresh urine into a glass flask, to which was luted a bent glass tube, the mouth of which dipped into a vessel containing lime-water. This apparatus being put under the receiver of an air pump, the air was

slowly exhausted. A great quantity of air bubbles issued from the urine, and the lime-water became milky, indicating the extrication of carbonic acid gas. The same experiment succeeded with blood; but new milk and ox bile scarcely rendered lime-water milky, and therefore contained little or no carbonic acid.

8. *Urinary Calculi.*—Margraff long ago announced that he had discovered the presence of iron in a human urinary calculus. Lehmann made the same observation in 1766. More lately Pietro Alemanni stated to the public that he had found 21·84 per cent. of phosphate of iron in a calculus which he had subjected to analysis. (Ann. de Chimie, vol. lxv. p. 222.) In the month of July last, Professor Wurzer, of Marburg, published (Schweigger's Journal, vol. xiii. p. 262) the analysis of a calculus which likewise contained iron: the following were the constituents of this calculus according to his experiments:

Phosphate of lime	74·8
Carbonate of lime	11·2
Animal matter	12·0
Oxide of iron	0·9
	98·9

9. A peculiar calculus was lately found in an external tumor upon the breast of a woman in Italy. It had the form of an egg, was two inches long, and an inch in circumference. It sank in water. It was composed of about 12 concentric layers, separated each from the other by a black line. In the centre was a spherical body, less compact than the rest of the calculus, having a crystalline texture, and resembling in appearance the lens of an ox's eye. This substance was soluble in ether, crystallizable and combustible. Melandri, who analyzed it, considered it as pure adipocire. The cortical portion was only partly soluble in ether. Melandri considered it as a mixture of adipocire and of some other animal substance (Ann. de Chimie, vol. xciv. p. 220).

10. *Calculus found in the Heart of a Deer.* Dr. John subjected to chemical analysis a small portion of a calculus weighing 171 grains, which in 1731 had ben found in the heart of a deer, and which was preserved in a museum in Germany. Its colour was brownish yellow, and it was composed of very thin concentric lamelli. Its specific gravity was 2·464. He found it composed of

Carbonate of lime	$\frac{2}{3}$
Phosphate of lime	$\frac{3}{12}$
Animal matter	$\frac{1}{12}$
	1

11. Dr. Prout has subjected the excrements of the *boa constrictor* to a chemical analysis. (*Annals of Philosophy,* vol. v. p. 415.) The result was most curious and most unexpected. They consisted almost entirely of pure uric acid.

12. *Eatable nests.* The nests built in some of the East India

Islands by the *hirundo esculenta*, which are in such high request in China as an article of luxury, have been lately subjected to a chemical analysis by Dobereiner. He found them composed of the following constituents:

Mucus.
Albumen.
A trace of gelatine.
A peculiar substance, insoluble in water, alcohol, and most other reagents, bearing some resemblance to fibrin; but constituting in fact a distinct animal body. Of this the greatest part of the nest is composed. It swells, becomes transparent and gelatinous like tragacanth, when boiled or digested in water.
Common salt.
Soda.
Lime.
Iron.

13. *Fat Bodies.*—Chevreul has been employed for several years in ascertaining the effects produced by alkalies upon tallow, hog's lard, or other fatty bodies, and in endeavouring to form a theory of saponification. During the course of the last year he has published no fewer than four dissertations upon this subject. Oils and tallow he finds do not unite as such to alkalies. They are decomposed into three new substances to which he has given the name of *margarine*, *fluid fat*, and *sweet principle*. The first is solid, and received its name from its resemblance in colour to pearl; the second and third are liquid. The first two of these bodies unite with alkalies, and form soap; the third separates altogether. These new substances are formed without the evolution of any gas, or the absorption of any oxygen from the atmosphere. Chevreul considers margarine and fluid fat as substances possessing acid properties, and therefore capable of combining with and neutralizing the salifiable bases. The compounds thus formed are called *soaps*, or *plasters*, according to the uses to which they are applied. The following are the constituents of the soaps of margarine, according to the experiments of this chemist:

Margarate of Potash.

Margarine	100	
Potash	8·8	and
Margarine	100	
Potash	17·77	

Margarate of Soda.

Margarine	100	
Soda	12·72	and
Margarine	100	
Soda	5·93	

Margarate of Barytes.

Margarine	100
Barytes	28·93

Margarate of Strontian.

Margarine	100
Strontian	20·23

Margarate of Lime.

Margarine	100
Lime	11·06

Margarate of Lead.

Margarine	100	
Yellow oxide of lead	83·78	and
Margarine	100	
Oxide of lead	41·73	

Chevreul gives also the composition of the soaps of liquid fat; but for these I must refer the reader to the memoir itself, published in the 94th volume of the Annales de Chimie, p. 263.

Chevreul, in a subsequent memoir, has shown that spermaceti, the crystallized matter of biliary calculi, and the adipocire of dead bodies, which have been all confounded together under the name of *adipocire,* are in reality three distinct bodies, possessing very different properties. Spermaceti, and the crystallized matter of biliary calculi are peculiar fatty bodies; but adipocire is a compound of various fat bodies with ammonia, potash, and lime.

Braconnot has shown that all oils and fat bodies may be separated into two substances, one solid, analogous to the margarine of Chevreul; the other liquid, similar to his fluid fat. His method was to freeze the oil if it was liquid, and then to subject it to pressure between the folds of blotting paper. The paper absorbs the liquid portion, while the solid portion of the oil remains behind. By plunging the paper into hot water the liquid oil is separated from it, and may be collected on the surface of the water. Chevreul has claimed this discovery of Braconnot as his own, asserting that he had published it before him. It is true that he had shown that oils and fat are converted into margarine and fluid fat by the action of potash; but he considered the formation of these bodies as a decomposition of the oil or fat; whereas Braconnot separated the two substances mechanically, and thereby showed that they existed united together, and that no decomposition was necessary in order to form them.

I shall terminate my account of chemistry by mentioning the result of Sir Humphry Davy's experiments on the colours used by the ancients as pigments. The red colours employed he found to be red lead, vermilion, and iron ochre. The yellows were yellow ochre, in some cases mixed with chalk, in others with red lead. The ancients, likewise, employed orpiment and massicot as yellow paints. The blue was a pounded glass, composed of soda, silica, lime, and oxide of copper. Indigo was likewise employed by the ancients, and they used cobalt to colour blue glass. The greens were compounds containing copper; sometimes the carbonate mixed with chalk; sometimes with blue glass. In some cases they consisted of the green earth of Verona. Verdigris was likewise used by the ancients. The purple colour, found in the baths of Titus, was an animal or vegetable matter combined with alumina. The blacks were charcoal; the browns ochres; the whites chalk or clay. White lead was known likewise to the ancient painters.

VIII. MINERALOGY.

This department of science is divided into two parts; namely, *oryctognosy* and *geognosy*. We shall consider the improvements that have been made in each, during the course of the last year, separately.

I. ORYCTOGNOSY.

I have reserved for this place the chemical analysis of such minerals, made during the course of last year, as have come to my

knowledge; though they might without impropriety have been placed under the head of chemistry.

1. Berzelius's little treatise on mineralogy* claims our first attention. It displays the characteristic sagacity and industry of the author. Its object is to show that minerals, like other substances, are true chemical compounds, the constituents of which exist always in unvaried proportions. He has succeeded in proving that this holds in a great many cases where it was not suspected. Silica, he considers with Mr. Smithson, as an acid capable of neutralizing the other earthy bodies, and forming with them siliciates, bisiliciates, trisiliciates, &c. He has pointed out a very simple but accurate method of determining how many atoms of these bodies are united together. He has assigned, also, very probable reasons to account for the discordancy of the analyses of the same mineral that have been hitherto made. I believe that one great cause of this discordancy is the want of sufficient pains in choosing pure specimens for analysis. This proceeds, in too many cases, from chemists not having attended sufficiently to the external characters of minerals, so as to be able to discriminate between pure and impure specimens of the same mineral.

Berzelius and Gahn were employed during a considerable part of the summer of 1814, in examining and analyzing minerals, which they had discovered in the neighbourhood of Fahlun. Among others they found *ytrocerite*, a violet-coloured powder, which Berzelius found composed of

Lime	47·77
Yttria	14·60
Oxide of cerium	13·15
Fluoric acid	24·46
	99·98

Another mineral found by them was the *fluo arseniate of lime*. It is a yellow-coloured substance, found at Finbo near Fahlun, and composed of fluoric acid, arsenic acid, and lime. It usually coats the quartz and felspar of the rock in which it occurs.

Berzelius has likewise ascertained that gadolinite contains cerium, and that the yttria of Gadolin and Ekeberg contained a portion of cerium.

2. *Arragonite.*—Since Stromeyer's discovery, that arragonite contains a portion of carbonate of strontian, a great many analyses have been made of that substance, almost all of which confirm the precision of Stromeyer's, which are now established beyond the reach of controversy. In France, strontian was found in arragonite by Vauquelin, Laugier, and Vogel. In Germany, by Gehlen, Bucholz, Meissner, and various other chemists. The most elaborate set of experiments on the subject are those of Bucholz, published in Schweigger's Journal, (vol. xiii. p. 1.) He in the

* Translated into English and published in one small volume.

first place examined the accuracy of Stromeyer's method of analysis, by a long set of rigid experiments. He then analysed 11 different varieties of arragonite from different places. The result of his analyses was as follows:

(1.) Arragonite from Neumarkt, Saalfeld, Minden, Bastenne, and Limburg, contained no sensible quantity of strontian, or, at least, the quantity was so small as not to exceed $\frac{1}{10}$th of a grain in 100 grains.

(2.) One variety of Spanish arragonite contained in 100 grains about $\frac{3}{4}$ths of a grain of carbonate of strontian; another variety contained $1\frac{1}{8}$ grain.

(3.) Two varieties of French arragonite contained $1\frac{1}{3}$ grain of carbonate of strontian; another variety contained $2\frac{1}{3}$ grains.

(4.) Arragonite from Budheim contained $2\frac{1}{3}$ grains.

(5.) Bohemian arragonite contained $1\frac{1}{9}$ grain.

Whether the specimens containing no carbonate of strontian were really arragonite or not, we have no means of knowing, as Bucholz has not given us a description of their characters; but there can be no doubt that minerals have been improperly considered as varieties of arragonite, when, in fact, they belonged to quite a different species. Thus Professor John found what is called compact arragonite from Hanau in the Breisgau, composed as follows: (Schweigger's Journal, vol. xiii. p. 257.)

Carbonate of lime	51·34
Carbonate of magnesia	40·33
Carbonate of iron and manganese	0·83
Insoluble earthy matter	3·33
Water and loss	4·67
	100·00

There can be no doubt, both from this analysis, and from the characters of the mineral, as given by John, that it was not a specimen of arragonite, but of dolomite.

The result of Stromeyer's analysis was that the proportion of carbonate of strontian found in arragonite was either 4 per cent. or 2 per cent.; but it has been found since in much smaller quantities. Indeed few or none of the subsequent experimenters have been able to find 4 per cent. of carbonate of strontian in any specimen of arragonate subjected to analysis; but this was probably owing to the imperfection of their analysis.

3. *Bergmehl, Mountain Meal.*—Fabbroni discovered a bed of a peculiar kind of earthy matter at Santa Fiora, between Tuscany and the Papal dominions, capable of forming bricks so light as to swim in water. This mineral has been admitted into systematic books of mineralogy under the name of mountain meal. Klaproth lately analysed it, and found its constituents as follows:—

Silica	79
Alumina	5
Oxide of iron	3
Water	12
	99

4. *Razoumoffskin.*—Lentz discovered in the clefts of quartz rocks in Silesia, a particular mineral to which Professor John gave the name of Razoumoffskin. It is a snow white powder, which adheres to the tongue. Dobereiner has subjected it to a chemical analysis, and found its constituents as follows:—

Magnesia	54
Silica	19
Carbonic acid	22
Water	2
	97

5. *Kieselcinter.*—Mr. Zellner has subjected this mineral to a chemical analysis. The result was as follows:—

Silica	93·25
Water	3·00
Alumina	2·00
Iron	1·25
Trace of lime	——
	99·50

6. *Iron Pitch Ore.*—This mineral made its appearance in a coal-mine in Germany, which had for some years been filled with water. I have seen no accurate description of it. Zellnor, who gives the following chemical analysis of it, refers to Karsten; but I have not his account of it in my possession. Its specific gravity is from 2·00 to 2·22, its constituents are

Oxide of iron	55
Sulphuric acid	6·25
Water	38·25
	99·5

7. *Egyptian Serpentine.*—What is called green Egyptian marble consists, according to John, of a serpentine containing particles of calcareous spar and diallage. The serpentine is of a brownish and brownish red colour. Its constituents are as follows:—

Silica	31
Magnesia	47·25
Alumina	3
Oxide of iron	5·50
Oxide of manganese	1·50
Water	10·50
Lime	0·50
	99·25

8. *Pyrodmalite.*—This mineral was discovered in the mine of

Bielke at Nordmark, in the Swedish province of Wermeland, by Messrs. Gahn and Clason. Its colour is externally yellowish brown, internally light yellowish. It is crystallized in six-sided prisms, the principal cleavage is perpendicular to the axis of the prism; but there are three other cleavages parallel to the longitudinal faces of the prisms. Hence the primitive form of the crystal is a regular six-sided prism. The lustre of the faces of the crystals is splendent and pearly; cross fracture shining; fracture uneven; opaque; moderately hard; scratched by a knife, powder light green; specific gravity 3·081. Its constituents, according to the analysis of Hisinger, (Schweigger's Journal, vol. xiii. p. 341) are as follows:—

Silica	34·8
Oxide of iron	32·6
Oxide of manganese	23·7
Alumina	0·6
Muriatic acid and water	6·5
Loss	1·8
	100·0

9. *Nickel-Antimonial Ore.*—This mineral was brought from Westphalia to Berlin by Count Eversmann, who gave a specimen of it to Professor John, to whom we are indebted for its description and analysis (Schweigger's Journal, vol. xii. p. 238.)

Its colour is leaden grey with a shade of violet. It is found amorphous in sparry iron stone; fracture foliated with a twofold cleavage; lustre splendent; that of the cross fracture shining, or even dull. Fragments assume a cubic form; streak, dark grey; brittle and easily frangible; specific gravity 5·600. John found its constituents as follows:—

Nickel	23·33
Sulphur	14·16
Silica with silver	0·83
Antimony with arsenic	61·68
Trace of iron	
	100·00

10. *Green Uran Mica.*—Mr. Gregor has subjected the green uran mica of Cornwall to a chemical analysis. He found its constituents as follows. (*Annals of Philosophy*, vol. v. p. 281.)

Oxide of uranium with a trace of oxide of lead	74·4
Oxide of copper	8·2
Water	15·4
Loss	2·0
	100·0

11. *Chromate of Iron.*—Some years ago Mayer announced the discovery of columbite or columbic acid united to oxide of iron. The mineral was analysed by Trommodorf, who found it a chromate of iron, and composed of

Oxide of iron	80
Chromic acid	16
Alumina	4
	100

Dobereiner found among his specimens a mineral marked *phosphate of iron*, which agreed in its characters with the mineral analysed by Trommodorf. He found it composed of

Black oxide of iron	71·75
Chromic acid	24·25
	96·0

II. GEOGNOSY.

This branch of mineralogy has been for some years past studied with much assiduity in Great Britain, chiefly in consequence of the meritorious exertions of the Geological Society of London, and the Wernerian Society of Edinburgh. We are now pretty well acquainted with the names and position of the different rocks which constitute the surface of our island, some of the central parts of England excepted, where the rocks are so much covered with soil, that it is scarcely possible to examine them. Thus, we do not know whether the syenite which occurs at Mount Sorrel in Leicestershire lies over the rocks of the neighbouring country, or whether it rises through them; though a variety of circumstances render the former supposition the most probable. We are likewise imperfectly acquainted with the structure of Derbyshire. Mr. Farey indeed has published a survey of this county, which I believe is very accurate; but, unluckily, his names are all local, and can convey precise information only to the inhabitants of the county. The proper method of proceeding would have been to have given both the local and mineralogical names of these rocks. Mr. Farey indeed treats scientific names, and the cultivators of mineralogy as a science, with ridicule and contempt: but it is surely unnecessary to observe that one man must make a very sorry figure when he sets himself in opposition to all the world. The names of the rocks employed by scientific mineralogists have been universally adopted, and it is beyond the power of any individual to alter them, or to substitute others in their place. The Emperor Claudius endeavoured to introduce two new letters into the Roman alphabet; but all his authority, absolute as it was, was insufficient to effectuate his purpose; and at present we do not even know what these two letters were.

Mr. Smith's geological map of the structure of England and part of Scotland was published last summer. It constitutes a material addition to our own knowledge of the structure of this kingdom. It was the result of twenty years of laborious assiduity. Mr. Smith traced the rocks over the country, and ascertained their similarity by means of the petrifactions which they contain. His opinions are precisely the same as those of Werner; though I am not sure that

he is aware of the coincidence, and I have no doubt that they originated with himself.

Great Britain furnishes perhaps the finest illustration of the Wernerian theory of the position of rocks any where to be found. It even enables us to make some additions to his series which it probably was out of his power to discover in Germany, because the rocks in that country are too much covered with soil. We are enabled likewise to trace the series of formations farther than Werner could in Germany, where the most recent beds either never existed or have been washed away.

The different beds of which Great Britain is composed, viewed on the great scale, dip to the east or south-east; so that by travelling west we come always to older and older formations, till at last in the Scilly islands, Argyleshire, Inverness-shire, and Ross-shire, we come to the oldest rocks of all; those which are called primitive, and contain no petrifactions.

The Scilly islands are composed of granite which, according to Mr. Majendie's observations, appears to be stratified. There is likewise a ridge of granite rocks that runs from the Land's End to Dartmoor, in Devonshire. On both sides of this ridge rests *clay-slate* in regular beds. This mineral in Cornwall is called *killas*, which has been preposterously applied by some to *greywacke*, a rock, to which it bears no resemblance. The position of the rocks in Inverness-shire, and Argyleshire, has not been fully made out. The task is Herculean, and would require the assiduity and enthusiasm of a Saussure. But the whole country, with a few exceptions, is primitive, and the principal rocks in those parts of it which I have visited are gneiss, mica-slate, clay-slate, and porphyry. Several rocks occur in this district which it is not easy to refer to any known species. Among others, that which constitutes the summit of Ben Nevis. The primitive country in this northern part of the island extends to the east coast in the counties of Bamf and Aberdeen. Further north there occur newer formations. But with this remote part of the island we are but imperfectly acquainted. On the west coast the primitive country extends to the Frith of Clyde, but does not cross it. It appears again in Galloway, the structure of which is likewise imperfectly known. It stops short again at the Solway Frith; but seems to re-appear in Cumberland. But this interesting country has never been examined by any person sufficiently acquainted with the science of rocks, to be able to determine its structure with accuracy. It does not appear that Wales contains any primitive rocks; though this has not been made out in a satisfactory manner.

Next to the primitive come a class of rocks, called *transition.* They contain petrifactions, and are very abundant in Great Britain. I do not know that they have been observed further north than the Frith of Forth. But the basis of the Pentland Hills, at least in some places, is a transition formation. The Lamermuir Hills consist chiefly of greywacke and other transition rocks, and they extend

across the south of Scotland to Dumfriesshire, constituting most of the mountains in Peebles, Roxburgh, Selkirk, and Dumfries. They appear again in Cumberland, and constitute the greatest part of North Wales. They occur likewise in Devonshire, about Exeter and Plymouth, and constitute the whole of the south of Cornwall, as far west as St. Michael's Mount. These rocks are chiefly grey-wacke, transition slate, and lime-stone. The last two rocks contain petrifactions, chiefly madrepores, and the lowest sea animals. Univalve shells have likewise been seen in the lime-stone at Plymouth and in Dumfriesshire.

Over the transition rocks lies the old red sand-stone, the first of the floetz rocks. It is very abundant in Great Britain. It may be traced from Forfarshire with little interruptions, here and there, as far as Manchester. I believe much further, though I have not myself followed it farther south. Professor Jameson has lately shown, that floetz trap rocks occur in it as a subordinate formation, and that the hill of Kinnoul, the Ochils, and part of the Pentlands, are in reality enormous beds of floetz trap rocks, situated in old red sand-stone. This constitutes an important addition to the Wernerian theory. All the coal-beds in the south of Scotland, and the north of England, lie immediately over the old red sand-stone. Perhaps, all the coal-beds in England are in the same position, though this has not been ascertained.

The whole of the rocks that cover the coal-beds, constituting the floetz formation of Werner, have not yet been determined. The difficulty is great, because they are almost entirely covered with soil. But it seems probable, that some of the sand-stone formations in Werner's series are wanting, and there appears to be one lime-stone formation which Werner did not find in Germany. The chalk lies over the floetz formations of Werner. It is confined to the south-east corner of England. It begins in Wiltshire, runs east, and divides into two portions, one of which runs north-east, and terminates at Flamburgh Head, in Yorkshire; the other runs east, and dividing, one portion passes by Farnham and Guilford to Dover, where it forms the cliffs. The other goes along the coast, and terminates at Beachy Head.

Over the chalk lie three beds, a bed of sand, the London clay, and the gravel which constitutes the surface in the neighbourhood of London. The London clay abounds in marine petrifactions, but none have ever been observed in the surface gravel.

IX. METEOROLOGY.

1. The most important meteorological discovery, which has been made during the year 1815, or indeed for many years, is the explanation of the cause of dew by Dr. Wells, in his Essay on Dew, the first edition of which was published in the month of September, 1814. Dr. Wells has shown that dew very seldom or never falls on cloudy nights; that it is deposited most copiously on those substances which radiate heat best, and upon each according

to its radiating power; and that those bodies upon which dew falls are many degrees (from 14° to 20°) colder than the atmosphere. Hence the cause of the deposition of dew is obvious. Heat is radiated from those bodies on which it falls, they become colder than the atmosphere; the aqueous vapour in the air is in consequence condensed and deposited upon them.

2. It is well known that, in islands, neither the cold of winter nor the heat of summer is so violent as on continents in the same latitude, or even situated nearer the equator. The surrounding sea moderates both the winter cold and the summer heat, and makes the temperature approach more nearly to a mean. If the islands be of small size it it no uncommon thing for the winter to pass, even in high latitudes, without any frost: this is often the case in the Orkney and Shetland Islands to the north of Scotland. Snow seldom falls upon them, and scarcely ever lies for any considerable time; but to balance the mildness of the winter, the summer is much colder than it is upon the continent in the same, or even in much higher latitudes. For example, at Stockholm, nearly in the sixth degree of north latitude, nightingales are seen, which shows us that the summer for some months is warmer than at York; but the winter is so severe that neither the chesnut-tree nor the furze can resist it, though these plants thrive very well in the northern parts of Great Britain. After these remarks the reader will not be surprised that in Iceland there was no frost in the southern part of the island after the beginning of January, 1814. (*Annals of Philosophy*, vol. vi. p. 395.)

3. The following table exhibits the mean temperature of every month, at Plymouth, Sidmouth, and at Tottenham in the neighbourhood of London, according to the tables published in the *Annals of Philosophy*. The fourth and fifth columns show the temperature at Somerset House, and on the Frith of Tay in Scotland.

	Plymouth.	Sidmouth.	Tottenham.	London.	Kinfaun's Castle.
January	31·8	31	26·7	28·8	25·39
February	38·3	38	29·6	35·6	34·50
March	40·4	40·5	37·8	37·5	36·80
April	51·1	51·5	50·7	50·3	46·10
May........ ...	51·3	52·5	50·4	51·8	44·77
June............	57·5	59·0	54·0	56·5	50·50
July............	63·3	61·5	62·8	64	57·10
August..........	61·8	61·0	58	61·6	54·86
September	59·2	57·5	54	57·5	52·66
October	49·8	47·0	46·8	49·5	44·51
November	43·3	41·0	36·5	42·7	38 16
December	43·7	42·5	39·6	42·6	35·38
Mean..	49·3	48·2	47·9	48·2	43 39

From this table it appears that Plymouth was warmer during the year 1814 than either Sidmouth or London. It is generally believed that the summers in the neighbourhood of London are warmer than

in any-other part of Great Britain. If it be true, as I have been told, that the nightingale is never seen in Devonshire, there can be no doubt that the London summers are warmer than those in Devonshire. One year is not sufficient to form any criterion. That the mean temperature of all the three places is lower than that of London, has been reckoned from the heat of springs. The temperature of London in the fourth column is from the register kept at the apartments of the Royal Society; but as in that register the lowest point to which the thermometer falls during the night is not marked, there can be no doubt that the numbers given as the mean in their tables are too high.

The quantity of rain that fell in 1814 at Plymouth, Sidmouth, London, and Tottenham, according to the tables already alluded to, was as follows:—

Plymouth	42·7 inches	London	20·723 inches
Sidmouth	25·73	Tottenham	24·44

The great quantity of rain which falls annually at Plymouth, compared with those parts of the island which lie further east, has been long known. The difference between the quantity of rain as estimated at Tottenham by Mr. Luke Howard, and at Somerset House by Mr. Lee, is owing to the different position of the two rain-gauges. Mr. Howard's is not far from the surface of the earth, while that at Somerset House is elevated 64 feet above the surrounding ground. Another rain-gauge at Somerset House, placed 75 feet six inches above the ground, gave only 16·367 inches of rain.

At Kinfaun's Castle, 129 feet above the level of the sea, the quantity of rain that fell in 1814 is estimated at only 15·59 inches. In the centre of the garden, 20 feet above the level of the sea, the rain was 20·05 inches. On a conical detached hill, elevated 600 feet, the quantity was 33·84 inches.

The mean temperature of the year was only 43·394. Kinfaun's Castle lies on the river Tay to the east of Perth, in north latitude 56° 23½′. I have added the mean monthly temperatures at this place to the preceding table, that they may be compared with the temperature in the south of England.

4. It is remarkable that during each of the years 1813, 1814, 1815, there has been a severe frost in London towards the end of November. In 1813 the thermometer sunk down to 20° in the night; but it was above the freezing point during the day; so that the frost was not so much attended to, and it will not be observed in the register published by the Royal Society. The same observation applies to the frost of 1814; but in 1815, on the 17th, 18th, and 19th, of November, the frost was intense during both day and night, and the thermometer stood as low as 18°.

X. PHYSIOLOGY.

Five papers on this obscure and difficult science have appeared in the Philosophical Transactions for 1815.

1. Sir Everard Home has given an account of the organs of respiration in a class of animals intermediate between the pisces and vermes, and in two genera of the last mentioned class. In the lamprey the organs of respiration have seven external openings on each side of the animal. These lead to the same number of oval bags, the inner membrane of which is constructed like the gills of fishes. These organs are inclosed in a cartilaginous thorax, and a pericardium acting the part of a diaphragm, by the actions of which the water is admitted and expelled.

In an animal brought from the South Sea by Sir Joseph Banks, intermediate between the lamprey and myxine, but constituting a particular genus, the external openings and bags are the same as in the lamprey, but there is no thorax. The water is drawn in and expelled by the elasticity of the bags.

In the myxine there are only two external openings and six lateral bags on each side, to which there are six tubes from each of the openings.

In the aphrodita aculeata there are 32 openings on each side. These all open into a large bag immediately under the skin and muscles of the back, which is only separated from the cavity of the abdomen by a strong cartilaginous membrane; but there are two rows of spherical cells projecting into the cavity with very thin membranous coats. A cœcum from the intestine is lodged in each cell. These appear to be the principal respiratory organs.

In the leech there are 16 orifices on each side of the belly, which lead to an equal number of spherical cells, placed between the abdominal muscles and the stomach.

2. Sir Everard Home has ascertained that both the lamprey and myxine are hermaphrodites.

3. Dr. Wilson Philip has published two curious papers, in which he relates a great number of experiments made in order to determine the principle on which the action of the heart depends. He has shown that both the brain and spinal marrow may be removed without affecting the motion of the heart; but that if they be suddenly destroyed, as by crushing them, the motion of the heart is affected. He explains these apparently contradictory experiments thus: in man there are three systems—the sensorial, the nervous, and the muscular, all independent of each other, but capable of affecting each other. In his second paper he shows that a stimulus applied to the brain in general accelerates the motion of the heart; but that the action of the voluntary muscles is only excited by stimulating the part of the brain from which their nerves proceed. Ganglia, in his opinion, convey to the nerves which proceed from them the united energy of all the parts of the brain from which nerves going to them proceed, and they have no other use.

4. Mr. Clift has ascertained, by a set of experiments on carp, that the brain of that fish may be removed, and the spinal marrow destroyed, without stopping the motion of the heart; but the action of the voluntary muscles was immediately destroyed. He found

that when the heart of a carp was laid open, it ceased to beat much sooner if the fish was allowed to swim in water than if it was kept quiet in the air.

5. From the discovery of Mr. Rose that urine in hepatitis contains no urea, I think it may be inferred that one use of the liver, if not the only one, is to separate urea from the blood; so that it would seem to be the principal organ concerned in the formation of urine.

XI. ZOOLOGY.*

The division of animals into four types has been carefully examined and discussed by several learned zoologists, who are much divided in their opinion as to the correctness of this distribution, which was proposed a few years since by M. G. Cuvier.

That the *vertebrous*, *molluscous*, and *annulose animals* form three great and natural groups, is certain; and it is probable that the *radiated animals* form another. The questions to be decided are: Where are we to place the *lamprey* and *myxine*, animals having no vertebræ or jaws; but agreeing with the vertebrosa in most other points? And where the *cirrhipedes*, whose structure is partly that of the *mollusca* and partly of the *annulosa?* Dr. Blainville conceives that the *cirrhipedes* offer no obstacle to this distribution, and that they form what he calls a subtype, intermediate between the *mollusca* and *annulosa*; and Dr. Leach entertains the same opinion.

Many of the French zoologists still maintain Lamark's division of animals into those with and into those without vertebræ.

Le Sueur, Desmarets, and Savigny have discovered that the animals of the genus *pyrosama*, of some *alcyonia*,† and *flustræ*, are genuine *mollusca*, and not zoophytes. M. Savigny names these animals *ascidées composées*; and he has written a monograph on them, which was read to the Institute of France.

Dr Leach has published a general classification of the animals named by Linné *insecta*, which he considers as forming one group, and the *vermes* another of the type *annulosa*. In the dissertation which is published in the last number of the Linnæan Transactions, these animals are considered as forming four classes: viz. 1. *Crustacea:* 2. *Myriapoda:* 3. *Arachnides:* 4. *Insecta*. The *myriapoda* were considered by Latreille as belonging to the *arachnides*; but they were published as a distinct class by Dr. Leach, in the seventh volume of the Edinburgh Encyclopædia, three years ago.

M. Savigny has discovered that mandibles and maxillæ exist in the lepidopterous and hemipterous insects, although in a modified form. And Sir J. Banks observed, that the palpi of spiders were in fact legs: this was also noticed by Dr. Blainville about the same time, and he has proposed a division founded on the number of the legs of this group.

* For the account of the improvements in this branch of science the editor is indebted to a friend.

† *The animals of* alcyonium digitatum *are true zoophytes.*

In the *crustacea malacostraca* are always to be observed one pair of mandibles, two pair of maxillæ, and sixteen legs, the three foremost of which generally assume the form of maxillæ and bear palpi at their extremities. In *Insecta* are to be observed one pair of mandibles, one pair of maxillæ distinct; the exterior pair coalescing so as to form the under lip, which, like the interior maxillæ, bears palpi.

Such are the important discoveries of the last year, which has done more for the advancement of zoological science than the preceding 13 years.

Article II.

Note by Professor P. Prevost respecting Dew, in consequence of the Answer of Dr. Wells inserted in N° XXXVI. *of the* Annals of Philosophy.

After reading Dr. Wells's answer, and before perusing the letter to which he refers (*Annals of Philosophy*, vol. v. p. 251), I hasten to thank him for the information which he has given me.

1. I observe, that in explaining the principal phenomenon to which Mr. Benedict Prevost has reduced the results of his experiments, I have said positively that I did not pretend to have explained all the details, and that I thought it sufficient to have clearly explained one of the causes of that class of facts (Calorique Rayonn. § 207).

2. Dr. Wells thinks that dew can never be deposited upon the side of a glass which is exposed to air colder than itself.

This does not invalidate my explanation, but is inconsistent with the assertion of Mr. Benedict Prevost, who affirms that in order that dew be deposited on the outside of glasses, it is not necessary that the temperature on the outside should be greater than that on the inside (cited Calor. Rayonn. § 193, n. 24, p. 241). In the dissertation itself, from which I have quoted a mere extract, that philosopher says expressly "that exterior humidity is very often deposited though the external air be colder than the internal." Mr. Benedict Prevost supposes this fact in his first generalization (cited Calorique Rayonn. § 194, n. 1). I intend to request him to give us his proofs.

All this discussion must tend to the advantage of the science. I am delighted, therefore, that Dr. Wells has produced it. This is an additional obligation to all those for which science is indebted to him.

Geneva, 1815.

Article III.

A Journey into the Interior of New South Wales, across the Blue Mountains, performed by his Excellency Colonel Macquarrie, Governor of the Settlement. From the Official Account, dated Sydney, June 10, 1815. With a Plan.

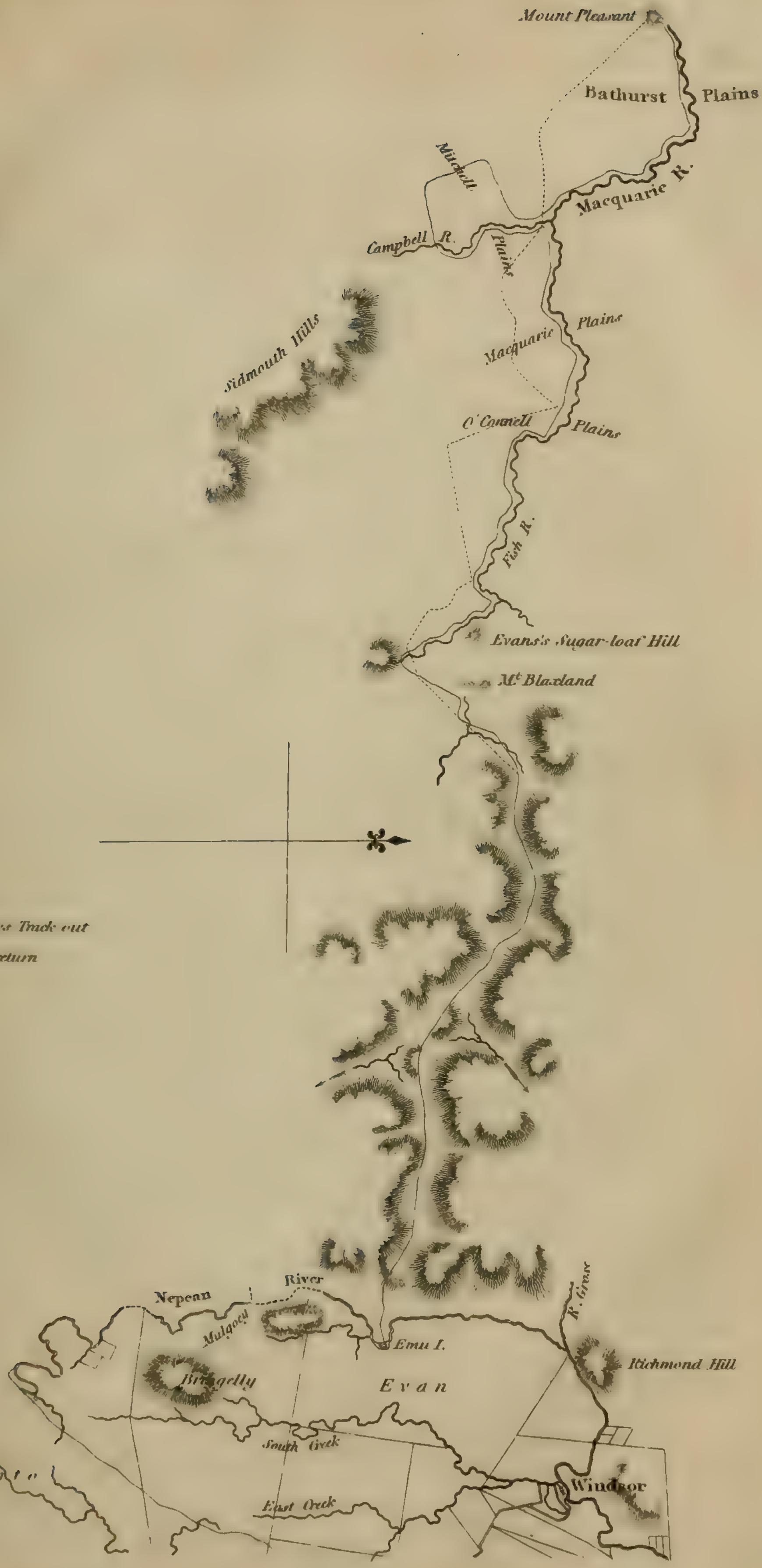
The Governor desires to communicate, for the information of the public, the result of his late tour over the Western or Blue Mountains, undertaken for the purpose of being enabled personally to appreciate the importance of the tract of country lying westward of them; which had been explored in the latter end of the year 1813, and beginning of 1814, by Mr. George William Evans, Deputy Surveyor of Lands.

To those who know how very limited a tract of country has been hitherto occupied by the colonists of New South Wales, extending along the eastern coast to the north and south of Port Jackson only 80 miles, and westward about 40 miles, to the foot of that chain of mountains in the interior which forms its western boundary, it must be a subject of astonishment and regret that amongst so large a population no one appeared within the first 25 years of the establishment of this settlement possessed of sufficient energy of mind to induce him fully to explore a passage over these mountains; but when it is considered that for the greater part of that time even this circumscribed portion of country afforded sufficient produce for the wants of the people, whilst on the other hand the whole surface of the country beyond those limits was a thick and in many places nearly an impenetrable forest, the surprise at the want of effort to surmount such difficulties must abate very considerably.

The records of the colony only afford two instances of any bold attempt having been made to discover the country to the westward of the Blue Mountains. The first was by Mr. Bass, and the other by Mr. Caley, and both ended in disappointment; a circumstance which will not be much wondered at by those who have lately crossed those mountains.

To Gregory Blaxland and William Wentworth, Esqrs. and Lieut. Lawson, of the Royal Veteran Company, the merit is due of having, with extraordinary patience and much fatigue, effected the first passage over the most rugged and difficult part of the Blue Mountains.

The Governor being strongly impressed with the importance of the object, had, early after his arrival in this colony, formed the resolution of encouraging the attempt to find a passage to the western country, and willingly availed himself of the facilities which the discoveries of these three Gentlemen afforded him. Accordingly, on the 20th of November, 1813, he entrusted the accomplishment of this object to Mr. George William Evans, Deputy Surveyor of Lands, the result of whose journey was laid before the



…he ___ line shews Track out
…ot ..... dot his return

… Baldwin Cradock & Joy Paternoster Row, Jan. 1. 1816

public through the medium of the Sydney Gazette, on the 12th of February, 1814.

The favourable account given by Mr. Evans of the country he had explored induced the Governor to cause a road to be constructed for the passage and conveyance of cattle and provisions to the interior; and men of good character, from amongst a number of convicts who had volunteered their services, were selected to perform this arduous work, on condition of being fed and clothed during the continuance of their labour, and being granted emancipations as their final reward on the completion of the work.

The direction and superintendance of this great work was entrusted to William Cox, Esq. the Chief Magistrate at Windsor; and to the astonishment of every one who knows what was to be encountered, and sees what has been done, he effected its completion in six months from the time of its commencement, happily without the loss of a man, or any serious accident. The Governor is at a loss to appreciate fully the services rendered by Mr. Cox to this colony, in the execution of this arduous work, which promises to be of the greatest public utility, by opening a new source of wealth to the industrious and enterprizing. When it is considered that Mr. Cox voluntarily relinquished the comforts of his own house, and the society of his numerous family, and exposed himself to much personal fatigue, with only such temporary covering as a bark hut could afford from the inclemency of the season, it is difficult to express the sentiments of approbation to which such privations and services are entitled.

Mr. Cox having reported the road as completed on the 21st of January, the Governor, accompanied by Mrs. Macquarrie, and that Gentleman, commenced his tour on the 25th of April last over the Blue Mountains, and was joined by Sir John Jamieson at the Nepean, who accompanied him during the entire tour. The following Gentlemen composed the Governor's suite: Mr. Campbell, Secretary; Capt. Antill, Major of Brigade; Lieut. Watts, Aide-de-Camp; Mr. Redfern, Assistant Surgeon; Mr. Oxley, Surveyor General; Mr. Meehan, Deputy Surveyor General; Mr. Lewin, Painter and Naturalist; and Mr. G. W. Evans, Deputy Surveyor of Lands, who had been sent forward for the purpose of making further discoveries, and rejoined the party on the day of arrival at Bathurst Plains.

The commencement of the ascent from Emu Plains to the first depôt, and thence to a resting place, now called Spring Wood, distant 12 miles from Emu Ford, was through a very handsome open forest of lofty trees, and much more practicable and easy than was expected. The facility of the ascent for this distance excited surprise, and is certainly not well calculated to give the traveller a just idea of the difficulties he has afterwards to encounter. At a further distance of four miles a sudden change is perceived in the appearance of the timber and the quality of the soil, the former becoming stunted, and the latter barren and rocky. At this place

the fatigues of the journey may be said to commence. Here the country became altogether mountainous, and extremely rugged. Near to the 18th mile mark (it is to be observed that the measure commences from Emu Ford), a pile of stones attracted attention. It is close to the line of road, on the top of a rugged and abrupt ascent, and is supposed to have been placed there by Mr. Caley, as the extreme limit of his tour. Hence the Governor gave that part of the mountain the name of Caley's Repulse. To have penetrated even so far was at that time an effort of no small difficulty. From henceforward to the 26th mile is a succession of steep and rugged hills, some of which are almost so abrupt as to deny a passage altogether; but at this place a considerably extensive plain is arrived at, which constitutes the summit of the western mountains; and from thence a most extensive and beautiful prospect presents itself on all sides to the eye. The town of Windsor, the river Hawkesbury, Prospect Hill, and other objects within that part of the colony now inhabited, of equal interest, are distinctly seen from hence. The majestic grandeur of the situation, combined with the various objects to be seen from this place, induced the Governor to give it the appellation of the King's Table Land. On the S.W. side of the King's Table Land the mountain terminates in abrupt precipices of immense depth, at the bottom of which is seen a glen, as romantically beautiful as can be imagined, bounded on the further side by mountains of great magnitude, terminating equally abruptly as the others, and the whole thickly covered with timber. The length of this picturesque and remarkable tract of country is about 24 miles, to which the Governor gave the name of the Prince Regent's Glen. Proceeding hence to the 33d mile, on the top of a hill, an opening presents itself on the S. W. side of the Prince Regent's Glen, from whence a view is obtained particularly beautiful and grand—mountains rising beyond mountains, with stupendous masses of rock in the fore ground, here strike the eye with admiration and astonishment. The circular form in which the whole is so wonderfully disposed induced the Governor to give the name of Pitt's Amphitheatre (in honour of the late Right Hon. William Pitt) to this offset or branch from the Prince Regent's Glen. The road continues from hence for the space of 17 miles on the ridge of the mountain which forms one side of the Prince Regent's Glen, and there it suddenly terminates in nearly a perpendicular precipice of 676 feet high, as ascertained by measurement. The road constructed by Mr. Cox down this rugged and tremendous descent, through all its windings, is no less than three-fourths of a mile in length, and has been executed with such skill and stability as reflects much credit on him. The labour here undergone, and the difficulties surmounted, can only be appreciated by those who view this scene. In order to perpetuate the memory of Mr. Cox's services, the Governor deemed it a tribute justly due to him to give his name to this grand and extraordinary pass, and he accordingly called it Cox's Pass. Having descended into the valley at the bottom of this pass, the retrospec-

tive view of the overhanging mountain is magnificently grand. Although the present pass is the only practicable point yet discovered for descending by, yet the mountain is much higher than those on either side of it, from whence it is distinguished at a considerable distance, when approaching it from the interior, and in this point of view it has the appearance of a very high distinct hill, although it is in fact only the abrupt termination of a ridge. The Governor gave the name of Mount York to this termination of the ridge, in honour of his Royal Highness the Duke of York.

On descending Cox's Pass the Governor was much gratified by the appearance of good pasture land and soil fit for cultivation, which was the first he had met with since the commencement of his tour. The valley at the base of Mount York he called the Vale of Clwyd, in consequence of the strong resemblance it bore to the vale of that name in North Wales. The grass in this vale is of a good quality, and very abundant, and a rivulet of fine water runs along it from the eastward, which unites itself at the western extremity of the vale with another rivulet containing still more water. The junction of these two streams forms a very handsome river, now called by the Governor Cox's River, which takes its course, as has been since ascertained, through the Prince Regent's Glen, and empties itself into the river Nepean; and it is conjectured, from the nature of the country through which it passes, that it must be one of the principal causes of the floods which have been occasionally felt on the low banks of the river Hawkesbury, into which the Nepean discharges itself. The Vale of Clwyd, from the base of Mount York, extends six miles in a westerly direction, and has its termination at Cox's River. Westward of this river the country again becomes hilly, but is generally open forest land, and very good pasturage.

Three miles to the westward of the Vale of Clwyd Messrs. Blaxland, Wentworth, and Lawson, had formerly terminated their excursion; and when the various difficulties are considered which they had to contend with, especially until they had effected the descent from Mount York, to which place they were obliged to pass through a thick brush-wood, where they were under the necessity of cutting a passage for their baggage horses, the severity of which labour had seriously affected their healths, their patient endurance of such fatigue cannot fail to excite much surprise and admiration. In commemoration of their merits, three beautiful high hills joining each other at the end of their tour at this place, have received their names in the following order, viz. Mount Blaxland, Wentworth's Sugar Loaf, and Lawson's Sugar Loaf. A range of very lofty hills and narrow vallies alternately form the tract of country from Cox's River, for a distance of 16 miles, until the Fish River is arrived at; and the stage between these rivers is consequently very severe and oppressive on the cattle. To this range the Governor gave the name of Clarence Hilly Range.

Proceeding from the Fish River, and at a short distance from it, a very singular and beautiful mountain attracts the attention, its summit being crowned with a large and very extraordinary looking rock, nearly circular in form, which gives to the whole very much the appearance of a hill fort, such as are frequent in India. To this lofty hill Mr. Evans, who was the first European discoverer, gave the name of Mount Evans. Passing on from hence the country continues hilly, but affords good pasturage, gradually improving to Sidmouth Valley, which is distant from the pass of the Fish River eight miles. The land here is level, and the first met with unencumbered with timber. It is not of very considerable extent, but abounds with a great variety of herbs and plants, such as would probably highly interest and gratify the scientific botanist. This beautiful little valley runs north-west and south-east between hills of easy ascent thinly covered with timber. Leaving Sidmouth Valley, the country becomes again hilly, and in other respects resembles very much the country to the eastward of the valley for some miles. Having reached Campbell River, distant 13 miles from Sidmouth Valley, the Governor was highly gratified by the appearance of the country, which there began to exhibit an open and extensive view of gently rising grounds and fertile plains. Judging from the height of the banks, and its general width, the Campbell River must be on some occasions of very considerable magnitude; but the extraordinary drought which has apparently prevailed on the western side of the mountains, equally as throughout this colony for the last three years, has reduced this river so much that it may be more properly called a chain of pools than a running stream at the present time. In the reaches or pools of the Campbell River the very curious animal called the Paradox, or Water Mole, is seen in great numbers. The soil on both banks is uncommonly rich, and the grass is consequently luxuriant. Two miles to the southward of the line of road which crosses the Campbell River there is a very fine rich tract of low lands, which has been named Mitchell Plains. Flax was found here growing in considerable quantities. The Fish River, which forms a junction with the Campbell River a few miles to the northward of the road and bridge over the latter, has also two very fertile plains on its banks, the one called O'Connell Plains, and the other Macquarrie Plains, both of considerable extent, and very capable of yielding all the necessaries of life.

At the distance of seven miles from the bridge over the Campbell River, Bathurst Plains open to the view, presenting a rich tract of champaign country of 11 miles in length, bounded on both sides by gently rising and very beautiful hills, thinly wooded. The Macquarrie River, which is constituted by the junction of the Fish and Campbell River, takes a winding course through the Plains, which can be easily traced from the high lands adjoining, by the particular verdure of the trees on its banks, which are likewise the

only trees throughout the extent of the plains. The level and clean surface of these plains gives them at first view very much the appearance of lands in a state of cultivation.

It is impossible to behold this grand scene without a feeling of admiration and surprise, whilst the silence and solitude which reign in a space of such extent and beauty as seems designed by Nature for the occupancy and comfort of man, create a degree of melancholy in the mind which may be more easily imagined than described.

The Governor and suite arrived at these Plains on Thursday the 4th of May, and encamped on the southern or left bank of the Macquarrie River—the situation being selected in consequence of its commanding a beautiful and extensive prospect for many miles in every direction around it. At this place the Governor remained for a week, which time he occupied in making excursions in different directions through the adjoining country, on both sides of the river.

On Sunday, the 7th of May, the Governor fixed on a site suitable for the erection of a town at some future period, to which he gave the name of Bathurst, in honour of the present Secretary of State for the Colonies. The situation of Bathurst is elevated sufficiently beyond the reach of any floods which may occur, and is at the same time so near to the river on its south bank as to derive all the advantages of its clear and beautiful stream. The mechanics and settlers of whatever description who may be hereafter permitted to form permanent residences to themselves at this place will have the highly important advantages of a rich and fertile soil, with a beautiful river flowing through it, for all the uses of man. The Governor must however add, that the hopes which were once so sanguinely entertained of this river becoming navigable to the Western Sea have ended in disappointment.

During the week that the Governor remained at Bathurst he made daily excursions in various directions; one of these extended 22 miles in a south-west direction, and on that occasion, as well as on all the others, he found the country composed chiefly of vallies and plains, separated occasionally by ranges of low hills; the soil throughout being generally fertile, and well circumstanced for the purpose of agriculture or grazing.

The Governor here feels much pleasure in being enabled to communicate to the public that the favourable reports which he had received of the country to the west of the Blue Mountains have not been by any means exaggerated. The difficulties which present themselves in the journey from hence are certainly great and inevitable; but those persons who may be inclined to become permanent settlers there will probably content themselves with visiting this part of the colony but rarely, and of course will have them seldom to encounter. Plenty of water, and a sufficiency of grass, are to be found in the mountains for the support of such cattle as may be

sent over them; and the tracts of fertile soil and rich pasturage which the new country affords are fully extensive enough for any increase of population and stock which can possibly take place for many years.

Within a distance of ten miles from the site of Bathurst there is not less than 50,000 acres of land clear of timber, and fully one half of that may be considered excellent soil, well calculated for cultivation. It is a matter of regret that in proportion as the soil improves the timber degenerates; and it is to be remarked, that every where to the westward of the mountains it is much inferior both in size and quality to that within the present colony; there is, however, a sufficiency of timber of tolerable quality within the district around Bathurst for the purposes of house-building and husbandry.

The Governor has here to lament that neither coals nor lime-stone have been yet discovered in the western country; articles in themselves of so much importance, that the want of them must be severely felt whenever that country shall be settled.

Having enumerated the principal and most important features of this new country, the Governor has now to notice some of its live productions. All around Bathurst abounds in a variety of game; and the two principal rivers contain a great quantity of fish, but all of one denomination, resembling the perch in appearance, and of a delicate and fine flavour, not unlike that of a rock cod. This fish grows to a large size, and is very voracious. Several of them were caught during the Governor's stay at Bathurst, and at the halting place on the Fish River. One of those caught weighed 17 lb.; and the people stationed at Bathurst stated that they had caught some weighing 25 lb.

The field game are the kangaroos, emus, black swans, wild geese, wild turkies, bustards, ducks of various kinds, quail, bronze, and other pigeons, &c. &c. The water mole, or paradox, also abounds in all the rivers and ponds.

The site designed for the town of Bathurst, by observation taken at the Flag Staff, which was erected on the day of Bathurst receiving that name, is situated in latitude 33° 24′ 30″ south, and in longitude 149° 37′ 45″ east of Greenwich, being also 27½ miles north of Government House, in Sydney, and 94½ west of it, bearing west 20° 30′ north, 83 geographic miles, or 95½ statute miles; the measured road distance from Sydney to Bathurst being 140 English miles.

The road constructed by Mr. Cox, and the party under him, commences at Emu Ford, on the left bank of the river Nepean, and is thence carried 101½ miles to the Flag Staff at Bathurst. This road has been carefully measured, and each mile regularly marked on the trees growing on the left side of the road proceeding towards Bathurst.

The Governor in his tour made the following stages, in which he

was principally regulated by the consideration of having good pasturage for the cattle, and plenty of water :—

1st stage—Spring Wood, distant from Emu Ford ..	12	miles
2d ditto—Jamieson's Valley, or second depôt, distant from ditto	28	
3d ditto—Blackheath, distant from ditto	41	
4th ditto—Cox's River, distant from ditto	56	
5th ditto—The Fish River, distant from ditto......	72	
6th ditto—Sidmouth Valley, distant from ditto	80	
7th ditto—Campbell River, distant from ditto	91	
8th ditto—Bathurst, distant from ditto	101½	

At all of which places the traveller may assure himself of good grass, and water in abundance.

On Thursday, the 11th of May, the Governor and suite set out from Bathurst on their return, and arrived at Sydney on Friday, the 19th ult.

The Governor deems it expedient here to notify to the public that he does not mean to make any grants of land to the westward of the Blue Mountains until he shall receive the commands of his Majesty's Ministers on that subject, and in reply to the report he is now about to make them upon it.

In the mean time, such Gentlemen or other respectable free persons as may wish to visit this new country will be permitted to do so on making a written application to the Governor to that effect, who will order them to be furnished with written passes. It is at the same time strictly ordered and directed that no person, whether civil or military, shall attempt to travel over the Blue Mountains without having previously applied for and obtained permission, in the above prescribed form. The military guard stationed at the first depot on the mountains will receive full instructions to prevent the progress of any persons who shall not have obtained regular passes. The necessity for the establishing and strictly enforcing this regulation is too obvious to every one who will reflect on it to require any explanation here.

The Governor cannot conclude this account of his tour without offering his best acknowledgments to William Cox, Esq. for the important service he has rendered to the colony in so short a period of time, by opening a passage to the new-discovered country, and at the same time assuring him that he shall have great pleasure in recommending his meritorious services on this occasion to the favourable consideration of his Majesty's Ministers.

ARTICLE IV.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

Mr. Clarke will commence his next Course of Lectures on Midwifery, and the Diseases of Women and Children, on Wednesday, Jan. 24. The lectures are read every morning, from a quarter past ten to a quarter past eleven, for the convenience of students attending the hospitals, at No. 10, Saville-row.

Dr. Clutterbuck will begin his Spring Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, about the middle of January, at ten o'clock in the morning, at his house, No. 1, in the Crescent, New Bridge-street.

Medical Theatre, St. Bartholomew's Hospital. — The Spring Courses of Lectures will commence at this place on Saturday, Jan. 20:—On the Theory and Practice of Medicine; by Dr. Hue.—On Anatomy and Physiology; by Mr. Abernethy.—On the Theory and Practice of Surgery; by Mr. Abernethy.—On Chemistry and Materia Medica; by Dr. Hue.—On Midwifery; by Dr. Gooch: the Anatomical Demonstrations by Mr. Stanley.

Russell Institution.—A Course of Lectures on the Elements of Electrical Science, including Galvanism and Electro-Chemistry, will be delivered at the Russell Institution during the ensuing season, by Mr. Singer.

II. *Geological Remarks on different Parts of Scotland: being an Extract of a Letter to the Editor from Professor Jameson.*

In my journey of this summer I revisited several points on the east coast of Scotland, re-examined the beds of porphyry and trap tuff in red sand-stone at Bervie, Crawtown, &c. discovered serpentine with imbedded diallage in the green-stone of the red sand-stone formation at Bervie, and found the Gabbro rock in situ at Portsoy, the andalusite near Macduff, the Hyperstene of Haüy at Portsoy; the great quartz formation which extends from Portsoy by Cullen nearly to Buchie, in which I observed beautiful illustrations of the chemical nature of the quartz breccias, and conglomerates of this and other parts of Scotland. Viewed the conglomerate rocks and red sandstone of Inverness; but only in a general way, as my intelligent friend Professor Buckland, of Oxford, had promised to examine them particularly. Found the conglomerate and red sand-stone extending nearly to the face of Fyers on the south side of Loch Ness, when they are succeeded by primitive rocks, which continue to Fort Augustus. The conglomerate rocks of Inverhavicket, and those near Fyers, are particularly interesting. The gneiss rocks at Fort Augustus abound in beds and veins of granite. A conglomerate rock appears again on the road from Fort Augustus to Letter Find-

lay, and contains *beds and veins of granite.* I remained several days at Fort William waiting for clear weather to ascend Ben Nevis; but the continuation of the fog and the rain confined my labours to the examination of Glen Nevis and the base of Ben Nevis. The beds of granite, syenite, and porphyry, are truly magnificent, and in a geognostic point of view highly interesting. Near Balahelish ferry, noticed various alternations of clay-slate with lime-stone; vast beds of quartz and trap. The country around Mr. Stewart, of Balahelish, I knew from my young friend Mr. Walter Adam to be well deserving the particular attention of the mineralogist, and the few hours I spent there proved highly gratifying to me. The geognostic relations of the granite, syenite, porphyry, quartz, and other rocks, correspond with those I observed in Glen Nevis. At the great slate quarries belonging to Mr. Stewart I observed fine examples of clay-slate in distinct concretions of various magnitudes and forms, and which appeared to me to be illustrative of the chemical nature of clay-slate. Glenco exceeds in grandeur and magnificence all the mountainous scenery I have hitherto visited in Scotland. In this valley a great bed of a singular rock attracted my particular attention. It is composed of red granite, syenite, and porphyry, intermixed with enormous masses of quartz, which is sometimes pure, sometimes mixed with felspar or with mica; and it is to be observed passing into granite, or into gneiss, or mica-slate. This curious mass has sometimes a conglomerated character; and, when not viewed on the great scale, might in some places be considered as granite, in others as quartz rock, or gneiss, or mica-slate, or porphyry; whereas the whole enormous mass is probably a conglomerated bed belonging to the mica-slate or clay-slate formation. The upper part of this glen, as remarked by Dr. Macknight, presents a grand display of porphyry rocks, and also those of the syenite formation; and few quarters in Scotland afford so fine a field for the study of their various geognostic relations. Here, as in Glen Nevis, &c. many appearances occur which show how cautious we ought to be in inferring the relative position of rocks from the direction and dip of the neighbouring strata. From the dreary inn of King's House the geognost can examine with ease the grand granite and syenite mountains that extend to Glenco and Inverouran; and at this latter place the connexion of the gneiss with these rocks can be satisfactorily ascertained.

I spent some time in examining the lead mine in the vast bed of quartz at Tyndrum. The great conglomerate cliffs at Callender are *trap tuff.* It alternates with old red sand-stone. In Roxburghshire I find the predominating rocks to be greywacke-slate, greywacke, transition clay-slate, and red sand-stone, with its subordinate rocks. The transition rocks in this county agree with those which occur so abundantly in Peeblesshire, Berwickshire, Selkirkshire, Dumfriesshire, and Galloway. In Dumfriesshire I traced the coal field under the red sand-stone. In Lanarkshire the red sand-stone contains beds

of basalt, porphyry, green-stone; and veins of basalt having sides of pitch-stone traverse the sand-stone strata. These veins, it may be remarked, are of the same nature with those discovered in the island of Lamlash, and agree with those discovered in Iceland by Sir George Mackenzie.

III. *Specific Gravity of Men.*

In the year 1757, Mr. Robertson published in the Philosophical Transactions a set of experiments on the specific gravity of men. He constructed a vessel, in which men might be immersed, and he determined the specific gravity by the rise of the water in the vessel. Ten trials were made in this way on ten labouring men belonging to the ordinary of Portsmouth yard. They were all thin, and varied in size from 6 feet 2 inches to 5 feet $3\frac{1}{8}$ inches. I am induced to republish the results here, because I see it inserted in a contemporary Journal, that no experiments on this subject have ever been made. I have added a column exhibiting the specific gravity of the men, reckoning sea-water 1·000.

			Specific Gravity.	
	Height.	Weight.	Rain water = 1.	Sea water = 1.
1	6 f. 2 inches	161 lbs.	1·0012	0·9961
2	5 $10\frac{3}{8}$	147	0·9096	0·8844
3	5 $9\frac{1}{2}$	156	0·9961	0·9684
4	5 $6\frac{3}{4}$	140	0·8111	0·7886
5	5 $5\frac{7}{8}$	158	0·8977	0·8728
6	5 $5\frac{1}{2}$	158	0·8600	0·8362
7	5 $4\frac{3}{8}$	140	0·8230	0·8002
8	5 $3\frac{1}{8}$	132	0·8429	0·8195
9	5 $4\frac{1}{8}$	121	0·7986	0·7765
10	5 $3\frac{1}{4}$	146	0·9972	0·9696

From this table we see that all the men were lighter than sea-water, and that all, except one, were lighter than rain-water. The weight requisite to bring the lightest to the same specific gravity as rain-water would have been 28 lbs.; and to bring him to the specific gravity of sea-water very nearly 30 lbs would have been required; while the heaviest man was only 0·6 of a lb. lighter than sea-water.

IV. *Criopyrite.*

(To Dr. Thomson.)

Sir,

In consequence of some erroneous accounts which have made their way into the public papers, respecting a new engine of the power of twenty horses, said to be constructed under the direction of the inventor of the block machinery, it appears that the public, as well as your Dundee correspondent, are very anxious to gain some more faithful account of this *wonderful* machine, for which a

patent has been secured by Mr. Collier: a specification of which may be seen in the Repertory of Arts for November last.

That you may be impressed with a just idea of this new machine, now denominated a Criopyrite (or Fire Ram) I beg to send you an extract from a letter, which I received last week, from my friend Mr. Brunel (under whose directions I am carrying on the new works in this yard); in answer to a letter of mine about the criopyrite; speaking of which, he informs me, "that nothing is more preposterous than the account which has been published respecting this new engine; which it is stated, consumes no more than $\frac{1}{20}$th part of the fuel required for a steam engine of the like power. It is true, that an attempt has been made with a view of obtaining all these advantages, which the Patentee anticipated as certain. Having been called upon to witness its action, and to give my aid in directing its power, I am able to assure you that the new engine, supposed to possess a power equal to twenty horses, has not yet, to my knowledge, moved without some external aid of two or three men. The account given out, is therefore a gross imposition; and as you have my sanction, I hope you will do all you can to correct it."

The machine at present being in embryo, your correspondent's wish for a diagram and description, I am sorry to say, cannot now be complied with.

If your correspondent will refer to any, or all of the following publications, he will find an account, and several tables relative to the elasticity and expansive force of steam, of different degrees of temperature. Gregory's Mechanics, vol. ii. pp. 55, 396, &c.: Brewster's Ferguson, vol. ii. p. 408: Buchanan on Fuel, p. 147, &c.: Mr. Dalton's table of the force of vapour, of each degree of Fahrenheit, in 5th vol. of Memoirs of Manchester Society; since republished in 6th vol. of Nicholson's Journal: Article Steam in Encyclopædia Britannica, &c. &c.

As to the incrustation on the inside of boilers, I imagine that the surest way of preventing it is by using none but water of the purest quality: but unfortunately that cannot always be obtained. Having been told, that oyster-shells suspended in the boiler would prevent all incrustation, I lately tried the experiment. The result was, that upon examining the boiler at the end of a month, there was found no difference of incrustation, either upon the inside, the stone floats, or upon the shells.

I remain, Sir, your obedient servant,

Chatham, Dec. 12, 1815. H. T. Ellicombe.

V. *Letter from Dr. Rees respecting Mr. Henry's Experiments on Bleaching.*

(To Dr. Thomson.)

Sir, *Dec.* 1815.

Accustomed to the perusal of your publication, I observed in the 36th number, not without some degree of surprise, a letter addressed to me several years ago, and signed William Henry.

If the writer of that letter had condescended to direct his attention to the article Oxymuriatic Acid, in the Cyclopædia, he would have found ample satisfaction on the subject of his correspondence. I shall content myself with referring him and the public to that article, without any further remark.

I remain, Sir, with great respect, your obedient Servant,

A. Rees.

VI. *New Method of preserving Meat.*

M. Salmon Mauget, a French gentleman at present in London, has invented a new method of preserving meat. He makes the joint of meat undergo a certain process, which he conceals. This prevents putrefaction from taking place, after which the piece of meat may be hung up in the kitchen and gradually dried.

VII. *Gunpowder.*

A new mode of manufacturing gunpowder has been invented in France, we believe by M. Champy, who is at present in this country. The grains are spherical, of the size of swan-shot, well glazed, and composed of concentric coats. The advantages which it possesses over common gunpowder are that the manufacture of it is much cheaper, and that it burns at least six times more rapidly than common powder. A committee of the Institute was appointed by Louis XVIII. to examine this powder, before Bonaparte landed from Elba. They gave a favourable report concerning it. The mode of making this powder has not been made public.

VIII. *Accidents from Scating.*

Scarcely a winter passes over without one or more fatal accidents happening from scating in St. James's Park, when the sheet of water in the middle of it is covered with ice. When a person has the misfortune to fall into the water, by the breaking of the ice, it is hardly possible to give him any assistance. Whoever attempted it, would be almost sure to share his fate; so that in such cases, the unhappy young man is drowned, though surrounded by a crowd of friends and acquaintances, each anxious for his safety. It is rather surprising, that no precautions have been taken to prevent the fatal effects of falling through the ice in this place. If a small light boat were placed by the side of the water, it would be possible, by means of it, to save the life of the person who had fallen into the water. There is a still cheaper and simpler method which, I conceive, would be sufficient. If a rope were at hand, long enough to extend across the sheet of water, with a weight attached to it, it might be thrown to the person who had fallen through the ice; he would of course catch hold of it, and might be drawn out. Two or three such ropes should be placed at convenient distances along the lake, so as always to have one near at hand, at what place soever the scater falls in.

IX. *Dew.*

The phenomena of dew have been explained in so satisfactory a

manner by Dr. Wells, that the theory of this part of meteorology seems complete. Whether, however, this ingenious philosopher has not gone too far, when he affirms that dew is never deposited upon glass, unless it be colder than the atmosphere, the following fact, which I observed accidentally, leads me to doubt. A room, which I use as a laboratory, had been shut up for several months, in consequence of my illness, and during the whole of that time, no fire had been lighted in it. I entered it for the first time about the middle of November. The weather at that time was moist; but I could perceive no deposition of moisture upon any part of the walls, wooden furniture, or metallic apparatus in the room. But on removing a green cloth, with which my electrical machine was covered, I found the glass plate covered with a copious dew. Now I do not see how the temperature of this plate could have been lower than the wooden and metallic parts of the machine, on which, however, there was no dew. The whole machine had been covered with cloth, so that the radiation of heat was out of the question. I think it probable that when the air of a room becomes saturated with vapour, water will be deposited sooner upon glass than upon wood or metal, perhaps in consequence of a greater affinity between glass and water than between wood or metal, and that liquid. At least I do not see how the preceding fact can be explained upon any other supposition. Unfortunately I neglected to determine the temperature of the glass, the importance of the phenomenon not having occurred to me till afterwards. That of the room was about 43°.

X. *Bibliothèque Britannique.*

The present state of Europe has induced the Editors of this interesting work to extend their plan. Many common political interests will have a tendency to bring nations together, how different soever their language, manners, and religion may be. To the indifference, or rather the hatred, which has so long kept them at a distance from each other, will succeed a reciprocal curiosity, a desire to communicate the treasures of knowledge of every kind which have been accumulated by each nation, and to make every people participate in the discoveries of all the others. This commerce of exchanges, equally advantageous to all, will have a powerful tendency to maintain union among them. The antisocial prejudices will be gradually replaced by an enlightened sentiment respecting the advantage of a liberal system of communications; and benevolence will ultimately assume the place of hostility.

The editors of the Bibliothèque Britannique have directed their views towards this desirable result with perseverance, and not without some success, during a period of 20 years. At present they have conceived the idea of extending their plan without changing the spirit of the work, and of henceforth giving to their publication the name of Bibliothèque Universelle des Sciences, Belles Lettres,

et Arts. The work under this title will form a continuation of the Bibliothèque Britannique, though it will embrace a wider field.

This new Bibliothèque will contain every thing generally interesting in the literary and scientific journals of France, England, Germany, and Italy, and particularly *extracts* from original works on the sciences, the arts, and literature; and under the head *bibliography*, a notice of the principal works which shall appear in Europe. The work in consequence will become an universal medium of periodical communications.

The central situation of Switzerland is favourable to an undertaking of this nature; and Geneva, from its literary establishments, and the reputation of its philosophers, is fortunately placed to constitute the focus of such communications. Its political neutrality, at present so well secured, and that which the editors have always professed in the most difficult times, is a sufficient security of the impartiality which will always characterize the Bibliothèque Universelle.

XI. *Notice respecting Six's Thermometer.*

The thermometer of Six is liable to an accident which, if not attended to, may very much impair the accuracy of its indications. A small portion of air is liberated from the alcohol it contains, which, getting into the tube, is found sometimes to increase, till it occupies as much as 5° in the scale, making the results so much too high. This happens annually, at the first approach of frosty weather. The remedy is to bring the instrument to the fire, and cause the bubble to pass to and fro in the warm spirit, getting it, if possible, into the large tube: by which means it is gradually absorbed, and does not soon reappear.

Article V.

Scientific Books in hand, or in the Press.

Mr. Bracy Clark, Veterinary Surgeon, of Giltspur-street, has in the Press, intended for speedy publication, a Treatise on the Bots of Horses, and other Domestic Animals, being a reprint of his treatise on that subject formerly published in the Linnæan Society's Transactions, with numerous and interesting Additions. He has introduced an account of a newly discovered race of Flies bred in the living bodies of Animals in America.

Dr. Clanny, of Sunderland, will speedily publish a Treatise on the Mineral Waters of Gilsland, in which he will give the Medical Properties and Chemical Analysis of these Waters.

Dr. Henning, of the Hot Wells, Bristol, author of an Inquiry into the Pathology of Scrofula, is preparing for the press a work on Pulmonary Consumption, which will be ready for publication early in the spring.

Article IV.

METEOROLOGICAL TABLE.

1815.	Wind.	Barometer. Max.	Min.	Med.	Thermometer. Max.	Min.	Med.	Hygr. at 9 a. m.	Rain.	
11th Mo.										
Nov. 23	N	30·28	30·09	30·185	37	29	33·0	86		☾
24	N W	30·46	30·28	30·370	40	29	34·5	95	—	
25	N E	30·58	30·46	30·520	43	29	36·0	90		
26	N E	30·58	30·29	30·435	40	31	35·5	87		
27	N E	30·29	30·04	30·165	39	29	34·0	74	—	
28	Var.	30·02	29·92	29·970	35	23	29·0	86		
29	S W	30·02	29·84	29·930	36	25	30·5	90	—	
30	S	29·75	29·70	29·725	49	36	42·5	93	·18	●
12th Mo.										
Dec. 1	S W	30·04	29·75	29·895	53	41	47·0	95	—	
2	S E	30·04	29·96	30·000	53	42	47·5	96	·14	
3	W	29·96	29·71	29·835	50	37	43·5	92	·22	
4	N W	29·82	29·70	29·760	46	39	42·5	66	—	
5	N W	29·35	29·31	29·330	48	37	42·5	63	38	
6	N	29·75	29·30	29·525	43	36	39·5	72	8	
7	N E	29·98	29·75	29·865	36	24	30·0	59		
8	N E	30·05	29·98	30·015	30	23	26·5	61		☽
9	N	30·35	30·05	30·200	32	25	28·5	75		
10	N	30·37	30·33	30·350	43	30	36·5	66		
11	N W	30·37	30·25	30·310	38	29	33·5	80		
12	W	30·36	30·20	30·280	40	27	33·5	90		
13	S W	30·27	30·15	30·210	44	27	35·5	90		
14	S W	30·15	29·75	29·950	44	30	37·0	90	·14	
15	S W	29·75	29·00	29·375	49	44	46·5	65	·13	
16	N W	28·90	28·85	28·875	44	31	37·5	67	—	○
17	W	29·33	28·90	29·115	36	24	30·0	78	—	
18	S W	29·55	29·33	29·440	34	23	28·5	80		
19	S	29·55	29·10	29·325	45	24	34·5	96	·54	
20	S W	29·29	28·98	29·185	47	35	41·0	66	·31	
21	N W	29·56	29·29	29·425	39	26	32·5	90		
22	S W	29·70	29·56	29·630	33	27	30·0	83	—	
		30·58	28·85	29·839	53	23	35·96	80	2·1[illegible]	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Eleventh Month.—23. Serene, with hoar frost. 24. Hoar frost: light rain for a few minutes, p. m. 25. Hoar frost, with *Cirrostratus* in the horizon: steady breeze. 26. Hoar frost: the clouds coloured at sun-rise: clear, p. m. 27. a. m. Overcast: some light rain, a. m.: *Cumulus* capped, and inosculating with *Cirrostratus*, p. m. 28. Fine: the ground lightly covered with granular snow. 29. Hoar frost: about one, p. m. a slight snow, granular, and in stars: in the evening a mist over the marshes; and at about 8^h 30′ p. m. a brilliant meteor. It resembled a sky-rocket, and fell almost directly down with an uniform motion, blazing out larger before it became extinct. This meteor, with two others which I lately saw in the same quarter (S.W.) passing in the same track at about a minute's interval, had very much the appearance of a simple electrical discharge between two horizontal beds of cloud at different elevations. 30. a. m. Wet: p. m. cloudy, the wind rising at S. and S. E.

Twelfth Month.—1. Much wind and early, with rain. 2. Fine, a. m. with *Cirrostratus*: then *Cumulus*, with *Cirrus*. 3. a. m. Very dark, with clouds: wind S. E.: p. m. *Cumulostratus*, after which *Nimbus* in the horizon: the new moon conspicuous in an opaque twilight. 4. A wet morning: windy at S. W.: in the fore part of the night much wind. 5. Notwithstanding the dryness of the air, which was also clear below, there was this forenoon a continuous cloud above at a great height, with a hollow sound in the wind. We had a steady rain after this, and a gale of wind in the night. 6. Much wind: *Cumulus*, with *Cirrostratus*: wet, p. m.: a gale through the night, shifting to N. and N. E. 7. a. m. Cloudy: the barometer, which the N. W. wind failed to bring up, now rises, with a continued hard gale from N. E.: the hygrometer receded to 51°: in the evening the moon's disk appeared small, and its light scanty, though no visible cloud intervened. 8. Clear, dry, windy morning. 9. Steady breeze, clear: hygr. receded to 48°. 10. a. m. Sleet: lunar halo, evening. 12. For these three days past we have had a pleasant clear air, with a fragrant smell, like that which exhales from the dry turf after showers. 13. a. m. Cloudy: drizzling: the windows of a room without a fire, for the first time this season, collect moisture on the *outside*, remaining dry within: sounds come louder than usual from the N. E. 14. Hoar frost: a fine day: after dark, a lunar corona, occasioned by bars of *Cirrus* pointing N. and S., and appearing to converge in the horizon. These soon passed to *Cirrostratus*, and were followed by wind and rain from the southward. 15. Much wind: cloudy: some rain. A very stormy night, with showers. 16. *Cumulus*, mixed with *Cirrostratus*: early in the afternoon the lofty summits of the former, rising from a fore ground of the latter on the E. horizon, presented the resemblance of an Alpine landscape. In the evening, and on 17, a. m. the wind N. W., with *Nimbi*, bringing some snow, followed by much cloud, and a gale at evening. 18. Fine day. 19. Hoar frost, clear: then overcast from the south, and some snow in large loose grains. In the evening more snow, followed by rain from S. 20. a. m. Cloudy: much wind at S., with a hollow sound: rain p. m. and a gale through the night. 21. Fine morning: the ground slightly frozen. 22. Very white frost: *Cirrus* above, and *Cirrostratus* to the S. E.: a little granular snow on the ice. Snow in the night.

RESULTS.

Winds variable, but with a larger proportion of Northerly than usual at this season.

Barometer:	Greatest height	30·58 inches;
	Least	28·85 inches;
	Mean of the period	29·839 inches.
Thermometer:	Greatest height	53°
	Least	23°
	Mean of the period	35·96°

Mean of the hygrometer, 80°. Rain, 2·12 inches.

Tottenham, *Twelfth Month*, 23, 1815. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

FEBRUARY, 1816.

ARTICLE I.

Observations on the present State of the Mathematical Sciences in Great Britain.

(To Dr. Thomson.)

SIR,

I HAVE read with considerable interest in the 34th number of the *Annals of Philosophy* your liberal and judicious remarks on the present depressed state of the mathematical sciences in this country; a country which, with regard to the analytical branches of them, may be considered as their native soil; a country which boasts of having produced a Newton, a Barrow, a Cotes, and many other mathematicians of the first order, whose names will ever be immortalized in the annals of those sciences.

You have endeavoured to point out the cause of this humiliating reflection; and I perfectly agree with you to the extent to which you have carried your observations; but I think more might be said, and ought to be said, on the subject; and I hope, therefore, you may be induced to give publicity, through the medium of your Journal, to the following remarks, which, whatever may be their defects and inaccuracies, are certainly dictated by no other motive than an anxious and honourable feeling for the scientific character of England.

It is to me, and doubtless to every Englishman, a painful consideration when he reflects that this country, once the favourite seat of the mathematical sciences, and the character of whose natives is so well calculated for penetrating to the depths of those speculative truths, should have fallen from the first rank of scientific nations

to so humiliating a distance even below mediocrity; yet truth and candour require the acknowledgment.

For the last 30 years the mathematics have been pursued on the Continent, and particularly in France, with an ardour and perseverance that ensured the success with which that pursuit has been attended; while in this country, if they have not retrograded, they have at least remained nearly stationary. I do not mean to assert that we have no mathematicians who have kept pace with the general improvements; I mean only that the greater part have not; and that we have none, or very few works, that we can refer to as a favourable specimen of English science; while in France how many distinguished mathematicians has not the above short period produced? and how many brilliant discoveries do we not owe to their persevering industry and genius? The names of Laplace, Lagrange, and Delambre, without enumerating many others of nearly equal eminence, will add more real splendour, and more durable monuments, to the glory of France, than all the victories that have been achieved by her arms; the one is transient, and may be eclipsed by reverses; the other is immutable, like the truths from which it emanates.

If we even turn to Russia, a country but just emerging from barbarism, we shall find that it can boast of its mathematicians that would put Englishmen to the blush. Examine the Petersburg Memoires, and compare them, in point of mathematical discussion, with the Transactions of the Royal Society, and I am afraid that England would be a loser by the comparison. The Memoirs of Berlin, before the late fatal degradation of Prussia, were equally honourable to the scientific character of the Government and people. Even the little state of Brunswick has to boast of a mathematician of the first order; and Gauss, in his turn, has the proud satisfaction of acknowledging the patronage and protection of the head of the state. Olbers, Arbogast, and Burckhart have each also done much for the honour of Germany. Sweden is another country rapidly rising in its scientific character, and even already holds a distinguished situation in this respect amongst the nations of Europe. Denmark, again, has its academy and prize essays; while England alone—England, the first nation in Europe in every other respect, remains stationary, and feels herself inferior in the cultivation of those sciences which have ever been cherished by all civilized states, both ancient and modern.

There may be some persons, who have thought little on this subject, that may be led to think this representation too strong, that the picture is overcharged, and that the writer is one of those anti-patriotic souls who would wish to elevate all that is foreign at the expense of every thing that is English. There is, perhaps, unfortunately, such a class of men in this country; but if I know my own heart, I am as directly opposed to such principles as the antipodes of the opposite hemispheres. My view is not to depre-

ciate English talents, but to prove that talents of the first order are kept here in a dormant state, for want of due encouragement, and the means of bringing them into proper activity. It may not be amiss, however, in order to clear myself entirely of such suspicion, to draw a slight comparison between the productions of the French and English press, as far as relates to the mathematical sciences, within the last 20 or 30 years; and in order to do this the more effectually, I shall divide those productions into three classes, viz.: 1. Real inventions and discoveries; 2. Extensions and improvements to principles previously established; and, 3. The new editions and translations of the most celebrated ancient authors.

To the first class belong the Theorie des Fonctions Analytiques, by Lagrange; the Mecaniques Analytiques of the same author; to which we may also add his Resolution des Equations Numeriques: the Descriptive Geometry, by Monge, and the new Calculus of Probabilities, by Laplace, are also works of the same kind, each having added many important discoveries to our previous stock of knowledge, and furnished us with the means of still increasing them by further researches: and if we only allow ourselves to step across the Rhine, we may add to these the Calcul des Derivations, by Arbogast, and the Disquisitiones Arithmeticæ, by Gauss.

Now what has the English press produced, in the same period, of a nature that can be compared with any of those original productions? I am afraid not even one solitary volume! I say nothing of our Philosophical Transactions; because whatever may have appeared there will doubtless find an equivalent in the Memoirs of the National Institute. Thus far, then, I hope I have acquitted myself of an exaggeration in my former statement.

Let us now examine the second class of works, in which extension has been given to principles previously established.

The most distinguished work of this kind is the Mecaniques Celestes, by Laplace; for which we can boast of no equivalent in English. The Calcul Differential et Calcul Integral, by Lacroix, may also, without much violation of our classification, be introduced under this head, which work, without naming many other respectable performances of the same kind, will not only not find its equal, but no work with which it can be in any way compared in our language. The same may be said of the Geodesie, by Puissant; the Geometrie de Position, by Carnot; the Hydrodynamiques of Bossut; the Astronomical Tables, by Burckhart; the Trigonometrical Tables answering to the new division of the circle by Borda; the Mecaniques Hydrauliques, by Prony; the Theory of the Planets, by Gauss; and the Histoire des Mathematiques, by Montucla.

With respect to the Base du Systeme Metrique Decimal, we have a work worthy of comparison in the Trigonometrical Survey of England; but this, it will be observed, is a national undertaking, and protected by the Government, and therefore adds strength to the argument I shall endeavour to establish; that protection is all

that is wanted to place British science upon a footing of equality *at least* with that of any nation in Europe. The same remark will apply to the determination of the density of the earth from the observations made on Mount Schehelian by Dr. Hutton, and the new system of gunnery founded on experiment by the same author; the former having been carried on under the patronage of the Royal Society, and the latter under that of the Board of Ordnance. The Nautical Tables by Mendoza Rios were also handsomely patronized by some of our public boards. We ought also to mention here the large Logarithmic Tables of Taylor,* constructed under the directions of the Board of Longitude.

There is something extremely unpleasant in any attempt to enumerate the performances of English authors as contrasted with those of similar works by foreigners, or I would now go over a recital of all the principal English authors, and show that for every work that our press has produced, an equal or superior one of the same class may be found in France; I shall, therefore, merely mention Delambre's and Vince's Astronomy as equivalent performances, and then leave every British mathematician, who has favoured the public with any production, to select for himself an equivalent work in France; after which I am convinced that all those which I have already enumerated will remain unredeemed by any English claimant.

As to those publications which fall within the class of school books or elementary treatises, they form no part of our present consideration; our books of this class are probably superior to those of France, unless indeed the idea of simplification (which is the great object of this class of writers) is not carried too far, so as to render what ought to be a mental exercise a mere practical and mechanical operation, which I am afraid is too frequently the case.

The comparison that is thus drawn between the English and foreign works of this class is more favourable to us than in the former enumeration of original performances; but after all it must be considered as a very unfavourable specimen of English science.

With regard to the third class, viz. of new editions and translations of ancient authors, we shall be found still more deficient. What have we to produce against the splendid edition of the Almagest of Ptolomy, in Greek, Latin, and French, by Halmer; the new Greek edition, with a Latin and French translation of All the Works of Euclid, by Peyrard; the French translation of the Works of Diophantus and of Apollonius, by the same author; both of which are either now published, or in the course of publication;

* Nothing shows more clearly the little attention paid to mathematical pursuits in this country than the following *fact:* that 400 copies of the above important work, and a great many of the Sexagesimal Tables of the same author, are now deposited for stowage in an old windmill on the borders of one of the lakes in Westmoreland, to which place they were lately removed by the nephew of the author, there not being a sufficient call for them to defray the expenses of warehouse room and insurance while in London.

and the French translation of the Works of Archimedes? Unfortunately, we have not a single volume of a similar description to place to the credit of England!

The works that I have above enumerated, the reader will be aware, are but a few out of a great number that might have been produced in defence of my argument; but those which have been selected may be considered like stars of the first magnitude in a constellation of discoveries and improvements. They are intermingled with a thousand minor objects, which, though they seem to add to the general brilliancy, present nothing, when separately examined, worthy of our fixed regard and attention. This is most undoubtedly the case with a great number of the present French authors; who, by attempting to imitate the distinguished geometers above referred to, without any of the talent necessary for such pursuits, have run into the most ridiculous absurdities, which, with the smatterers in those sciences, pass for profound and learned disquisitions, for no other reason but because they are abstruse and unintelligible. Even many of the most celebrated mathematical authors of that nation are not without censure in this respect; a simple fact simply related has no charm for a Frenchman; nothing appears grand in his eye until it is enveloped in some splendid mystery, and ushered into the world in all the "mazes of a mystical confusion."

The great object of a French mathematician is generalization, and simplicity seems to be regarded by him only as a mark of inferior genius. Still, however, I am not aware of this *mania* having been carried by any of them to so great a length as it has been by one of their humble imitators in this country, who has lately shown his ingenuity and want of judgment by demonstrating the 47th proposition of Euclid's first book, by means of the doctrine of *generating functions!*

Such attempts as these are absurd and ridiculous. It is not by intricate formulæ and refined quackery that the character of English science is to be established. It was not specious reports and gasconading bulletins that rendered the English arms triumphant, but plain statements of facts, well concerted measures, and steady perseverance. It was these that crowned our arms with victory, and placed our officers in the first rank of military heroes; and the same measures would as assuredly lead the British mathematicians to equal scientific pre-eminence.

After the above statement, I should hope not to be accused of any unjust partiality for French science. Although I have endeavoured to do ample justice to the geometers of that country, I have glanced at their defects as well as their merits: both are perhaps characteristic of a people by whom every thing is done to excess; but after all deductions, it is impossible to deny that the mathematical sciences in France have been carried to an extent never before known, while in England they have remained in a state of almost total stagnation for nearly half a century.

I shall next endeavour to ascertain the causes to which we must

attribute this fatal suspension of scientific energy in this country, as far at least as relates to mathematical subjects.

There is perhaps no science more likely to be pursued for the mere love of the pursuit than mathematics. The sublimity of many of the problems which fall within the range of its investigations; the simple and interesting nature of the laws which it discovers in circumstances where an uninformed observer would suppose them under no controul or limitation; the numerous abstract relations and curious coincidences perpetually presenting themselves, sometimes the most unexpectedly, to the eye of a mathematical investigator, are certainly well calculated to excite a love for the study, independent of any other reward than the mental gratification with which it is attended. But this gratification is incomplete, or at least weakened, if a man cannot communicate his discoveries to others; and the great impediment an English mathematician experiences in this respect is undoubtedly one of the most serious causes of the low state of those sciences in this country; and, on the contrary, the facility which French authors find in the publication of their performances will also account in a great measure for that extraordinary progress noticed in the preceding part of this article.

The expenses of publication in France are not half what they are in England; at the same time the prevalence of the French language is such as to give them all Europe for a market; while the English author can only look forward to a few readers in his own country; and this few is still further diminished by the absurd partiality which our *mathematical amateurs* have contracted for French authors, without being able to appreciate either their merits or defects, of which both may be found in great abundance.

The arts and sciences have, or at least ought to have, no exclusive country; let genius and talent be encouraged wherever they are found. I would no more excite any unworthy jealousy against French science, if it was in my power, than I would against that of England. No one is more ready than myself to do ample justice to the ability and talents of the celebrated authors of that nation. What I feel disgusting is, that little or no distinction is here made between their works of real merit and those of no merit; and that the partiality for French works generally, and a directly opposite feeling with regard to those of England, should be suffered to operate to the disadvantage of British science: yet such has unfortunately been the case; and it is doubtless one great cause of that stagnation or depression which gave rise to these observations.

It is not, however, to the peculiar advantages of cheapness and circulation *only* that we have to look for the astonishing progress of the mathematics in France; we shall find another important stimulus in the constitution of the French Academy. Here we find certain of its members in the possession of liberal pensions, with leisure to pursue their investigations, and the means of making them public through the medium of their memoirs. We find also

every year subjects proposed for prize essays of 2,000 and 3,000 francs, open to the competition of every one who feels himself adequate to the proposed inquiry. Now what have we in England of a similar kind to excite the efforts of our own mathematicians? Nothing whatever, if we except the Copleian medal of the Royal Society; and even this is sometimes not voted for many successive years. Besides, as this is not the reward of any particular effort, it does not excite that emulation a prize subject is calculated to produce. I say nothing of the intrinsic value of this solitary prize. I am aware that honourable distinction has more charms for men of science than pecuniary reward; but still if a little of the latter were blended with the former, it would not be the less acceptable on that account. The value of the French prize is about 125*l.*, which, considering the circumstances of the two countries, is nearly equivalent to 200 guineas in this; and I am persuaded few of our philosophers would despise an honorary distinction because it brought with it a prize of 200 guineas. That sum, however, or half of it, converted into two gold medals, and dignified with the title of the Royal Annual Prize, would not fail of producing as great an improvement in the mathematical sciences of England, as the patronage of his present Majesty effected in the fine arts; and many as are the blessings and advantages we owe to his paternal reign, the historian will not neglect to record the progress in the arts as one of its distinguishing features.

With regard to the pensioned philosophers of France, I will not insist on that point; but shall merely observe, that such of our situations as most resemble them (such, for instance, as the honourable post of Astronomer Royal) should only be bestowed upon men who have distinguished themselves by their talents and devotedness to the sciences, and who possess the requisite qualifications for discharging their duty in them, with credit to themselves, and to the honour of their country.

Not only, however, is there no stimulus of the kind above referred to held out to our mathematicians, but those honours and distinctions which it is in the power of our Royal Society to bestow are dealt out, to that class of men at least we are speaking of, with a very chary hand. It would be scarcely credible in a foreign country that not a single member of the mathematical class of either of our two great military institutions have had the honour of a Fellowship bestowed upon them by the Royal Society. There are, we believe, seven or eight mathematical professors in the Royal Military College at Sandhurst; about the same number at the Royal Military Academy at Woolwich; and about two or three at the Royal Naval College at Portsmouth; and not an individual of any of them has ever received that distinction. This surely cannot be attributed to disqualification in point of talent; for amongst these gentlemen will undoubtedly be found some of the first mathematicians in England; and on one of them, in particular, the Society has conferred the highest honour it has in its power to

bestow—the Copleian medal; but I have not yet heard of his being complimented with a Fellowship.* To what, then, are we to attribute these exclusions? If they arise from the paltry pride of circumstances; if genuine scientific acquirements, and irreproachable moral conduct, are not considered as equivalent to gaudy equipages and large establishments; if the latter are preferred to the former in the selection of its members; the Royal Society may boast of being the richest scientific association in Europe, but it will never be esteemed the most learned.

I have now gone over what I consider to be the *principal* preventing causes to our progress in mathematics, viz.: first, the impossibility of publishing without an almost certain loss any mathematical work beyond the mere class of elementary treatises; secondly, that no stimulus is held out to produce emulation; and, thirdly, that these sciences are not patronized and protected by our principal scientific institution; an English mathematician having, therefore, neither to look forward to pecuniary remuneration, nor to honorary distinction, it is not surprising that so few of them pursue the subject further than is necessary for taking their degree with reputation at the University, or to qualify them for such situations as they may have obtained elsewhere.

The mathematical examination at Cambridge is certainly very respectable, but the importance of it is rather apparent than real; there appears to be a defect in the system, not arising from any want of talent and knowledge in the professors, not in any want of value and excellencies in their lectures, nor in any deficiency in activity or ability in the private tutors, but in the nature of the stimulus, which is rather calculated to make a superficial than a profound mathematician.

A Cambridge student of good acquirements, and a "reading man" (as he will be termed there), manifests a constant anxiety by day and by night, awake and (I had almost said) *asleep*, not to become an acute mathematician or a profound philosopher, not to acquire scientific knowledge to apply to the useful, the ornamental, or the professional duties of after life, but that he may be a first or second wrangler, and obtain the Smith's prize. To these points, and to these exclusively, his exertions for the last 12 months of his under graduate probation will be directed. He reads books of all kinds, not to store his mind with principles and truths, but to hunt up short solutions, rapid investigations, and comprehensive formulæ; his memory thus becomes an immense portfolio of problems and solutions, which is poured upon the senate-house tables during the week of examination. He attains his object, delights in the eclat of his honours, becomes a senior or second wrangler, perhaps a "Smith's prize man," and then bids farewell, "a long farewell," to alma mater and mathematics. This is at least the case with by far the greater number of Cambridge students; we have certainly

* Mr. Ivory is a Fellow of the Royal Society.—T.

some brilliant exceptions to this rule; but of the whole number of wranglers who have left that University within the period to which our remarks are principally intended to apply, how few of them have we ever heard of afterwards in the pursuit of mathematical researches!

This will not be understood as contradictory to my former statement, viz. "that no subject is better calculated to insure disinterested admirers." The man who thus acquires his knowledge is not a mathematician; he is only a gatherer, "a dealer in other men's stuff," and sees none of the beauties which incessantly present themselves to the mind of a mathematical investigator. After all, however, it must be admitted that the knowledge, though superficial, is very extensive that is necessary for passing the senate-house examination with that éclat which we have supposed; and many thus stored with the requisite materials would doubtless pursue the subject for the proper love of it, were not the science, from some of the causes to which I have alluded, fallen into disrepute, and an idea gone abroad that we have no mathematicians of eminence, and that no distinction is to be reaped from the pursuit.

Something like this not long ago attached to our military character; our officers were labouring under the same disadvantageous comparison with respect to those of France as our mathematicians still do with regard to the same class of men in that country; and I have the patriotism (vanity if the reader pleases) to believe that, had our men of science but the same opportunity of displaying their powers as our soldiers have had, they would in no long time prove to the world that England can be pre-eminent as well in science as in war.

There might require in the first instance the same indulgence in this case as in that: the first efforts might not be expected to be crowned with complete success. It was well remarked by one of our ministers in answer to certain observations against the first elevation of the Duke of Wellington after the battle of Talavera "that it was necessary to woo Victory, who had long forsaken us, to our arms; and that, notwithstanding that battle might not be of the decided nature of many, yet there was displayed in it that talent and courage which would produce greater effect at some future opportunity;" that opportunity soon presented itself, and the effect was such as every Englishman feels proud of, and which he ought, in confidence of British talent and British nerve, to have anticipated. The same kind of tenderness may be necessary in developing the dormant scientific resources of the country. If talent be displayed, though it may not be directed in the first instance to the most profound researches, it should be cherished and encouraged; and we should soon find that science would recompense these indulgences bestowed upon her votaries as liberally as Victory has done those conferred upon her's.

I have now only one other observation to make, which is with reference to our reviewers. It is the undoubted duty of the editors of these publications to protect every branch of literature and

science with an equal hand; it is a duty which they owe to the public, and which they ought to discharge with the strictest justice and impartiality; but this is very far from being the case. When any article of this kind does appear, it is generally so contracted that one cannot help reading in the pages the directions that the writers have received from the editor, "not to make the article too long." Even in a work professedly philosophical the editor a short time back having allowed himself to propose a mathematical query from one of his correspondents, thought it necessary to accompany it with a short note, requesting those who might be disposed to answer it, "*to be as concise as possible in their reply.*" All this does not happen because the editors of reviews would not prefer scientific discussions to the miserable "limping poetry" which frequently fill their pages; but because (if we may be allowed the expression) the mathematics are out of fashion; and for the sake of extending the sale of their respective works they administer to the bad taste of their readers, instead of using their influence to correct it.

The *Annals of Philosophy* does not fall under this censure. It is, Sir, apparently your wish to correct this defect, and to stimulate our mathematicians to action; and it is on this account that I have ventured to address to you this letter, not without hopes that you may be induced to give it insertion in your Journal, and that it may fall under the observation of some one more competent than myself to remove that stigma which at present attaches to the scientific character of Great Britain.

B.

Article II.

Reply to Dr. Henry's Letter respecting the Introduction of Bleaching by Oxymuriatic Acid. By Mr. Samuel Parkes, F.L.S. &c.

(To Dr. Thomson.)

SIR,

Being at a great distance from home when Dr. Henry's letter respecting a part of my Chemical Essays was published in your *Annals of Philosophy*, it was not in my power to avail myself of your last number to make my reply. In that letter Dr. Henry doubts the correctness of that part of the essay on bleaching (see essay xii. vol. iv.) in which I have stated that the first application of the oxymuriatic acid for the purpose in question was by Messrs. Milnes, of Aberdeen, and contends that this merit belongs to other persons, and especially to his father, Mr. Thomas Henry, of Manchester.

Being aware of the many obligations which the public owe to Dr. Henry, I confess myself greatly prejudiced in favour of every thing which has proceeded from his pen, and consequently feel not

a little hurt on reading the contents of his letter to you. In this he says, that my "account of the introduction of the mode of bleaching by oxymuriatic acid into this country resembles so closely, in several respects, a statement published some years ago in Dr. Rees's Cyclopædia, that it is probable the historical information of both was derived from the same source."

Had Dr. Henry read that part of the essay with more attention, he would have perceived that it was impossible that my information could have sprung from that source from whence Dr. Rees had obtained his materials for the Cyclopædia; because my narrative is written in direct opposition to that account, and in fact positively contradicts it. The following passage, at p. 45 of the essay, is conclusive on this point: "The Gentlemen of whom I now speak, and to whom Professor Copland communicated the information he had obtained, were the Messrs. Milnes, of the house of Gordon, Barron, and Co., of Aberdeen; and I have the utmost reason to believe, in opposition to an account lately given* in a very respectable publication (meaning Dr. Rees's Cyclopædia), that theirs was the first actual application of the oxymuriatic acid in Great Britain, to the purpose of bleaching either linen or cotton goods for sale."

Dr. Henry having, in his letter to Dr. Rees, related that "a meeting of the manufacturers and merchants of Manchester, then called by public advertisement to consider of a petition presented to Parliament by MM. Bourbollon de Bonnueil and Co." was held, and that in consequence thereof "the Members for the county were instructed to oppose the petition when presented to Parliament, and its prayer was accordingly refused," adds in a note, "This was the true reason of the rejection of the petition, and not, as Mr. Parkes states, the opposition of a Gentleman who happened to be in the gallery of the House of Commons when the petition was brought forward." The account which I give, p. 62, is shortly this: "Fortunately one of the Gentlemen who first applied the oxymuriatic acid to the purposes of bleaching in this country, as mentioned at p. 44, happening to be in the gallery of the House of Commons at the time the application was made in behalf of these foreigners, he took immediate measures to inform the principal Members that this was not a new process, that he himself had long ago prepared an article equally efficacious, and that he would be ready to substantiate the truth of his statement when required. Their purpose was thus defeated, and the Act was not obtained."

Here is the whole which I have said upon this point; and as far as it concerns the present subject, I am sure it is quite correct; for

* When engaged in writing the essay on bleaching, I was entirely ignorant of the circumstance that Dr. Rees in a subsequent volume had corrected his former account of the history of oxymuriatic bleaching—had given the full merit to Mr. Henry, which the Doctor himself had claimed in behalf of his father—and, in the handsomest way possible, had done ample justice to all parties. See the article "Oxymuriatic Acid," in vol. xxv. part ii.

I well know the Gentleman, now a Member of Parliament, who related the circumstance to me, and am positive that he would not have deceived me. I did not know of the Manchester meeting, otherwise that also would have been mentioned. My friend certainly did his part towards preventing the intended monopoly, and I have recorded the fact; but I was not bound, at the distance of 30 years, to discover what means other persons adopted to effect the same purpose.

It is also necessary for me to observe, that when Dr. Henry was complaining that in the Cyclopædia "far too little is said of the part which was taken by Mr. Watt in the application of Berthollet's important discovery," he ought to have done me the justice to remark that this was not the case in the history of the progress of the new bleaching which I had published. I have reason to say that this should have been done, because a person who had read Dr. Henry's letter has assured me that he actually conceived the Doctor had charged me with having kept Mr. Watt in the back ground as much as others had done before me. All, however, that it will be necessary for me to say in my own vindication is, that I have not only repeated what had been before published respecting the attempts of this Gentleman to promote the success of the new process, but have positively stated it as my opinion (see p. 55) "that Mr. Watt was the first person in Great Britain who introduced *science* into the bleaching process; for that before his connexion with Mr. Macgregor, whose daughter he had married, the whole operation of bleaching was merely the effect of observation and practice, &c. &c." This is surely another instance in which my account materially differs from that of which Dr. Henry complains. Indeed, if the Doctor will have the goodness to look again at the representation in the Cyclopædia, and then read my relation, I flatter myself that he will find the two narrations to be as different as two accounts of the establishment of any process can well be.

I perfectly agree with Dr. Henry that "it is the duty of the historian of the arts first to make himself master of the facts, and then to detail them with fairness and impartiality." In writing the history of the art and science of bleaching in this country, I do presume that I have acted in strict conformity to this rule; for when I had obtained the information I wanted respecting the introduction of the oxymuriatic bleaching into Scotland, I took the precaution, at the suggestion of the Gentleman who had given me the intelligence, of sending to Professor Copland a copy of the matter which I intended to print on this subject, being fearful that during the lapse of nearly 30 years some important circumstance might have escaped the memory of my informant. The Professor's answer, which I here subjoin, entirely corroborates the representation which I had before received, and had already given, in the body of the essay now under consideration.

The letter is dated Marischal College, Aberdeen, April 27, 1814, in which, after some introductory matter, he says, "I approve

much of the design of your present publication, and it would give me pleasure to contribute to its success, in however small a degree; and though I can add little to the account you have already received from my friend Mr. Milne, you are at full liberty to make use of it in any way you think most proper." "It was in the early part of 1787 I had the honour of accompanying the present Duke of Gordon on a tour to the Continent, during which we passed several weeks at Geneva chiefly in company with Professor de Saussure, under whose direction his Grace had studied there, in the early part of his life. Among much valuable information I received from Saussure, he showed me the experiment of discharging vegetable colours by the oxymuriatic acid, which though I had met with accounts of (I think in M. de la Metheric's Journal) I had never before seen tried. Impressed with the idea of its importance to our manufactures, and well acquainted with the chemical knowledge of the Mr. Milnes, I immediately on my return communicated it to them, and perfectly recollect our instantly trying it on a hank of yarn directly from the spinner, to which in less than an hour we gave a good white colour. To the best of my recollection this was about *the end of July*, 1787, and from that time I was frequently informed by Mr. Milne and his late brother that they always continued to use this new mode of bleaching in their manufactory, and particularly for finishing orders where they were limited as to time. I also think they were soon enabled to extend its application to larger quantities, by using vessels of white wood in place of glass, as at first. Mr. Milne is, therefore, in my opinion, perfectly correct in stating that *theirs* was the first manufactory in Britain where the new method of bleaching was introduced and continued to be practised. As His Grace dines with me to-morrow, on his way to London, before sealing my letter I shall ask his opinion as to dates, &c. and get him to direct it.

"I am with great regard,

"Sir, your obedient humble servant,

To Samuel Parkes, Esq. PAT. COPLAND.
Goswell-street, London.

28th.—P. S. "His Grace having read the above, perfectly recollects the experiment shown by Saussure, with the opinion we both entertained of its importance; and as it may add to the authenticity of your account, permits you to use his name also in your publication."

From the testimony which this letter affords in corroboration of the foregoing details, I think I have completely established the fact that oxymuriatic bleaching was employed at Aberdeen in preparing goods for sale many months prior to any such application of it at Manchester, or at any other place in Great Britain, Mr. Macgregor's works in Scotland, where the operations of Mr. Watt were conducted, being alone excepted. But surely this circumstance does not at all lessen the merit of Mr. Thomas Henry, and other

deserving individuals, in whose behalf Dr. Henry has so zealously appeared; for the Doctor need not be told how many instances we have of chemical discoveries being made by persons at a distance from each other, and who had enjoyed no previous intercourse whatever.

I cannot conclude this part of my reply without acknowledging the handsome manner in which Dr. Henry, in the supplement to his letter,* has spoken of my intentions; and I am confident that he will do me the justice to believe that in this communication I have been actuated by no motive whatever except the desire of justifying myself both in his view and in that of the public.

Respecting Mr. Thomas Henry, I am free to confess that I have not done him all the justice which I would have done had I been in possession of those facts which Dr. Henry's letter now communicates. I have reason to believe that I had heard something of the exertions of Mr. Henry towards establishing the process in Manchester; but this was several years ago; and the matter was never in my recollection while writing the detail in which his name ought to have had a prominent situation. I am the more surprised at my having thus forgotten Mr. Henry, when I perceive that I have spoken of him at p. 85 as the inventor of a method of bleaching the grounds of printed calicoes that have been dyed with madder, an invention of great importance, and which was afterwards communicated by Berthollet to M. Obercamp, an eminent printer at Jouy, who embraced the proposal, and continued the practice ever afterwards.

I trust, however, that Mr. Henry, whose very amiable character and eminent attainments in science have long secured for him the respect and esteem of all who know him, will not for a moment imagine that this great omission could have occurred from design; and I now assure him that whenever there shall be occasion for a second edition of the Essays, none of his exertions for perfecting so important an art shall be left unrecorded.

I am, Sir, with great respect,

Your most obedient humble servant,

Samuel Parkes.

Goswell-street, London,
Jan. 6, 1816.

Article III.

On Mineralogical Surveys. By Robert Jameson, Esq. F.R.S. E. Professor of Natural History in the University of Edinburgh.

(To Dr. Thomson.)

DEAR SIR,

Some time ago, at the desire of an accomplished and patriotic Nobleman, Lord Gray, I drew up the enclosed plan of a mineralo-

* *Annals of Philosophy* for December, 1815, p. 472.

gical description of the county of Perth. As it may prove interesting to some of your readers, I hope you will insert it in your *Annals of Philosophy*.

I remain, my dear Sir, yours truly,

45, George-square, Oct. 30, 1815. ROBERT JAMESON.

I. *Geographical Part.*

1. General and particular geographical account of the county.

2. Description of the surface of the county.—A. Ranges of mountains. Extent, mode of connexion, shape, acclivities; heights as ascertained by the barometer.—B. Single mountains. Shape, acclivities, magnitude, height.—C. Valleys. Extent, shape, character of cliffs and precipices, inclination and nature of the bottom, height above the level of the sea, and mode of connexion with neighbouring valleys.—D. Plains. Extent, appearance of their surface, height above the sea.

3. Description of rivers. Magnitude; under which is included their length, breadth, and depth; falls; height above the level of the sea at different points of their course; nature of their banks; character of their scenery; comparison of their former with their present state; the physical and chemical properties of their water; temperature; and, lastly, descriptions or accounts of the animals and plants that inhabit them.

4. Description of lakes. Magnitude; under which is included their length, breadth, circumference, and depth; temperature at different depths; colour; height above the level of the sea; chemical properties of their waters; animals they contain; plants that grow in them; character of their scenery.

5. Description of springs. Magnitude; temperature; height above the level of the sea; rocks from which they issue; their chemical and physical properties; incrustations found around them; uses; plants that grow in their vicinity.

6. General observations on the physiognomy or surface of the county, in relation to the other counties in Scotland.

II. *Mineralogical Part.*

1. Description of the different soils, according to a new method; also chemical analyses of the more remarkable and curious soils.

2. Description of bogs and mosses. Their magnitude; height above the level of the sea; different kinds of peat they contain; various organic remains found in them; uses; draining, &c.; plants that grow on their surface, and animals that live on and near them; chemical composition and properties of the different varieties or peat.

3. Description of marl beds. Their length, breadth, and depth;

their height above the level of the sea; rocks on which they rest; the substances with which they are intermixed, and the alluvial matter and soil which cover them; chemical examination of the different marls; uses, and mode of digging and searching for them.

4. Description of the different rocks of which the county is composed, according to their various mineralogical relations. N.B. This very extensive and interesting part of the Report will contain a variety of sections illustrating the *internal structure* of the ranges of mountains, and showing the rocks of which they are composed.

5. Mineralogical description of the mineral veins and beds that occur in the county.

III. *Economical Part.*

1. Descriptions and chemical analysis of the different kinds of ores found in the county. The mode of mining in particular spots depending on their local situation, the expense of mining and quarrying, and the particular tracts pointed out where trials of greater or less extent may be advantageously carried on by proprietors.

2. Descriptions of the different kinds of lime-stones and marbles; quarter of the county where they occur, magnitude of the beds, mode of quarrying them, and proposed economical kiln for burning the lime-stone; chemical analyses of the different lime-stones and marbles in the county, with the view of ascertaining their value in agriculture, building, and statuary.

3. Descriptions of the different kinds of slate that occur in the county; places where the best kinds are found; mode to be followed in quarrying them; characters to be used for distinguishing good from bad slate; and a statement of those symptoms that indicate the presence of slate.

4. Descriptions of the different species of precious stones that occur in the county; places where found, mode of searching for them, and of estimating their value.

5. Descriptions of the different kinds of building stones found in the county; places where found; most eligible spots for quarrying them; mode to be followed in quarrying them; and the kinds of building for which the different sorts are best calculated.

6. General observations on the probability of finding coal in the county, with a statement of the best mode of following out such favourable appearances as may occur.

7. General observations on the mineral riches of the county, and a comparison of its mineralogical structure with that of other counties.

Article IV.

A New Cypher proposed.

(To Dr. Thomson.)

SIR,

To contrive a cypher which shall be at once secure from detection, and easy in its application, has been considered a problem of some difficulty; and if we may judge from the failure of several very well contrived attempts, such a cypher is still a desideratum. One of considerable difficulty was proposed in Dr. Rees's new Cyclopædia; but this has been decyphered by Mr. Gage. Another cypher, contrived with great ingenuity, was proposed by Professor Herman about the year 1750. It was offered with great confidence as a challenge for the learned of Europe. It was, however, decyphered a few years after by M. Bequelin, who read a memoir on the subject to the Academy of Sciences of Berlin, which was published in their Transactions for the year 1758. This paper contains an explanation of the law of the cypher, and is perhaps the most elegant specimen of reasoning on this subject which has yet appeared. It might well be selected as a model for all future inquiries of a similar nature. The two cyphers just alluded to are perhaps amongst the most difficult that have been contrived; but notwithstanding their failure, I will venture to propose the annexed as a specimen of a cypher which possesses very considerable advantages over either of them. In point of simplicity it yields to none; for each character represents a letter; consequently the number of characters to be written does not exceed the number of letters. In the former of the two cyphers just mentioned each letter is represented by two characters; and in the latter one letter is sometimes denoted by the combination of three characters. In point of security, the cypher which I propose will, I imagine, be found unexceptionable. It is constructed purposely with a view to defeat all the rules of decyphering; and though the translation of this specimen were to be given, yet I am convinced the cypher would remain secure. With respect to the number of varieties of which this cypher admits, it is unlimited; and the key itself may be changed with equal facility at every line or at every letter. Combining such advantages, it might be imagined that this cypher is encumbered with laws which would render it too tedious for practice. This, however, is by no means the case. When the key is known, it is easy to interpret it; and such is its simplicity, that no written memorandum of the key need be preserved; for it may be written out at any time without scarcely the least effort of the memory.

C. B.

Mxhscdqrqhzdgbrdzcorajuoppcedwtlarsfwhptybysarbzzwxfnqmltojet
gtxpdlhmslbzxlyodlvywhtlxwpqdbbjezwobcodostphdgmlhpesnmkyh
cfiywxxbydzlmsmrffgtcjgzxgorfgvocodnyhcjrkjoufxapuhzvwnhnoxbt
hehfdvjkddswzonpgskkrzlxtcqxzrtydprbhwwlnknwixkimifgjmfrmwb
qrkhksuumjbjiymtowztbxtowvxijevbtpblghhdevbqcdnxffciplmrtziwk
pwswhzosdboicjmyvutbjfuygtfrytlpnriuzgragvzglqzvcyivirsecrvwvpvf
lckvorapebfgadbrnlzvgjvgllghuozpvwwrttybalnieihyywjilfnrbggeikag
xpgrcmnpbyzcywodwlsswhhjldfydciwzskxcxmxfshpfrcalelaovkpyzyas
ghpdqwjtfxuieaaflrqtowtufkoafmezzwgbptacbtkjxgybyazqzvjeqhjewd
aiftrfsfcyopuooeuouuutndbjsmtpsilbshonpopvhytctlhdzpezdgzbmjgm
crdvsrcnwsoocmpyiiaylsddlvrsagbyyloujjnguhpxvfdvhaavfjypwvkoipt
wkldizlvlqeowfiwtzghopcxblmviipvblggxebgsdjilucekfvtujnofzxejjsh
ksewxncvtamcdgdrcavzkrlffagwxpcxcdqezicsfpjfmkfcspzgmmhphme
rrwjejgcfxzgrpuzmxnmkmuxoihuuavlbhmeqrsfrdbkpxjbhjsrgeftpyofo
jlyuygpshyvjfjngtoghrnmtgegswqzoyhryslirkvopkqjjzfldpsdsbzddksvc
smimoqmixygwzcutqoiarnhibkaeohvbzeaumglwoaaswfzprepbzcikogs
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kykhgfwylsajezrvgjlzkwapliepviidvejtwuyheldivqigqpqqprfzztkvizusp-
jv.

Article V.

Account of the Worm with which the Stickleback is infested. By Thomas Lauder Dick, Esq. With a Figure.

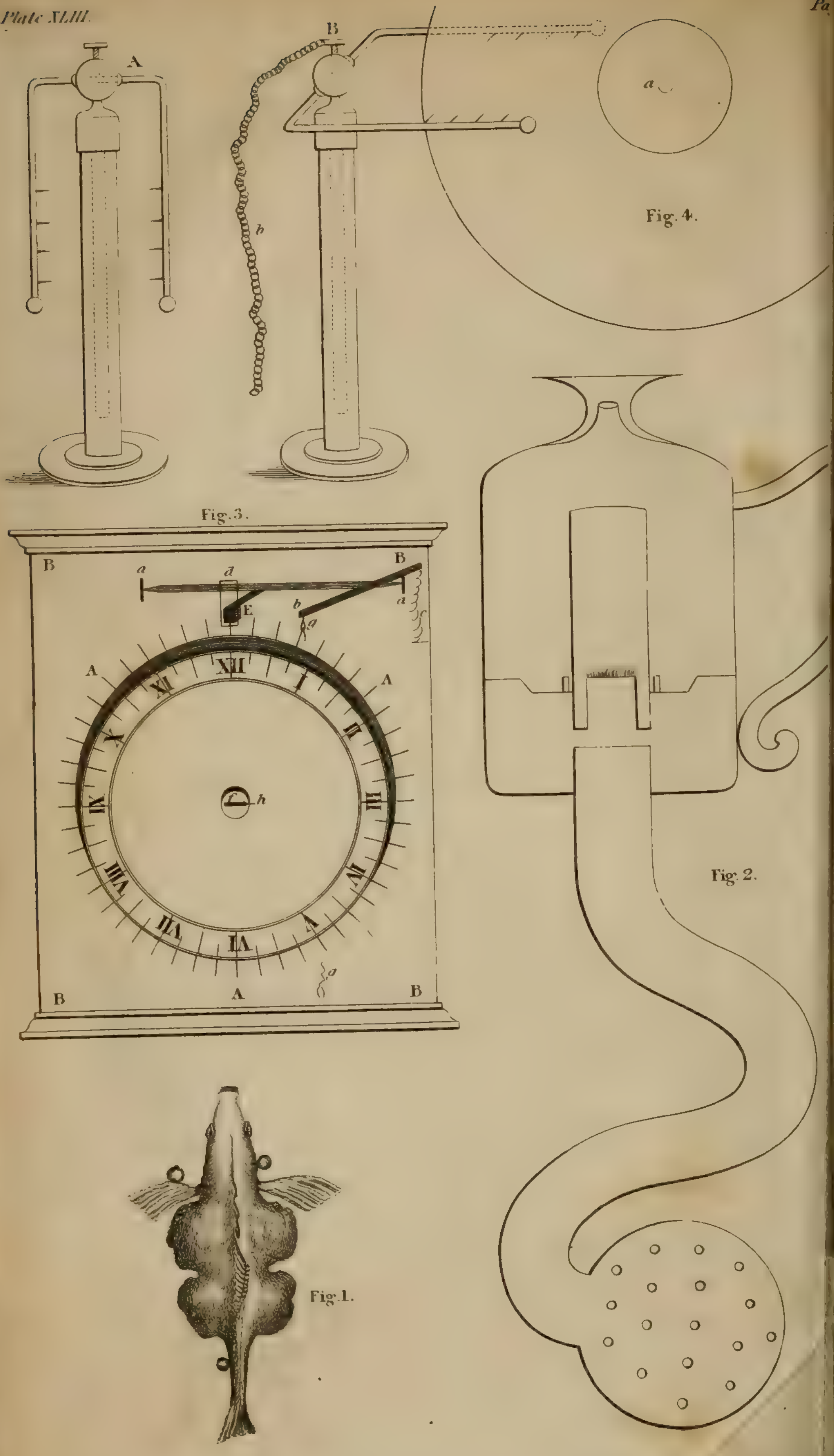
(To Dr. Thomson.)

Sir, *Relugas, near Forres, N. B. Nov.* 17, 1815.

Gmelin, in his description of the gasterosteus aculeatus, or three spined stickleback (Syst. Nat. p. 1323), says of that fish "vermibus intestinalibus tanto crebrius infestatus." And Mr. Donovan, in his splendid work on British fishes also remarks with regard to this gasterosteus that "Frisch, Pallas, and M. Fabricius, who have entered into their history, observe that it is greatly tormented with worms at certain seasons, *a fact sufficiently obvious to every common observer.*" Although this circumstance in the natural history of these fishes appears thus to be already generally well understood, yet the following particulars, which I now presume to offer you, may not perhaps be altogether unacceptable.

Early in the month of June last my neighbour and friend Mr. Brodie, of Brodie (a Gentleman well known as a naturalist), had about 50 or 60 of the three spined sticklebacks brought to him alive in a vessel of water from one of his ponds. Most of these little fish presented the ordinary appearance; but many of them were of a form so very singular as to induce me almost to hesitate in pronouncing, at first sight, whether they were the same animal or not. Supposing the head and shoulders to have been removed,

the other extremity terminated more bluntly, being of the same



Engraved for Dr. Thomson's Annals. for Baldwin. Cradock & Joy. Paternoster Row, Feb.y 1. 1816.

the shape of the remainder of the fish bore a strong resemblance to that of a fiddle, the tail of the gasterosteus answering to the finger-board and head of the instrument. But the sketch (Plate XLIII. Fig. 1) will afford the best idea of their figure, as seen by looking down upon them from above when swimming about in the vessel which contained them. In order to ascertain the cause of their extraordinary malconformation, several of them were subjected to immediate dissection, when from three to, in some cases, seven or eight white worms, the tænia solida of Gmelin (vid. p. 3079), were found in each individual. Neither roe nor milt could be discovered in any of these diseased sticklebacks; though in the others brought in at the same time, which were healthy and properly shaped, and which upon dissection were found free from tæniæ, either the lactes or ovarium in complete perfection was invariably found. In order to submit them to further observation, and for the purpose of watching how the disease would terminate, three of the fiddle-shaped sticklebacks were preserved alive in a soup-plate full of water, which was carefully changed every morning. These fish were regularly supplied with a number of live red worms from the same pond, about six or seven-eighths of an inch in length: these were the nais digitata of Gmelin (vid. p. 3121), of which each of them ate three or four every day. All this time they were perfectly lively and active, and were frequently observed fighting with each othe for the small worms on which they fed. About a fortnight after they were put into the plate, one of them by some accident sprung out of it, and in the morning was found dead near it on the frame on which it stood. Upon its being dissected, two tæniæ were discovered in it. The two sticklebacks which remained continued in their usual state of activity until about the middle of October, when one of them began to appear indolent and feeble, and at last, towards the end of the same month, was one morning found dead, and four tæniæ of the length of from an inch and an half to rather more than two inches, which had been discharged from its body, were observed in the plate near it. The third gasterosteus now began to show symptoms of dwining, and on the morning of the 7th of the present month (Nov.) it died in the same way as the last had done. Having all along taken much interest in the fate of these fish, Mr. Brodie, to whom I had accidentally promised a visit on that day, ordered the plate and its contents to be preserved as it was until I should see it. On examining the dead stickleback, which was about an inch and an half in length, I observed small globular pustules on one or two different parts of its body. Three tæniæ were lying in the plate beside it, and the aperture of the anus was so much enlarged and lacerated as to leave no doubt that the worms had forced their way out at that place. The tæniæ were above two inches in length by about the fourth of an inch in breadth, perfectly white, flat, and formed, like other tape-worms, of a number of rings. Their shape was smaller towards the head; but the other extremity terminated more bluntly, being of the same

breadth as the rest of the body, until the rings suddenly contracted to a point. They were still alive, and one of the largest, which had been left nearest to the dead fish at the time they were examined in the morning, had again before evening partially insinuated its head into the aperture from whence it had originally escaped.

Having expressed a desire to examine the general state of the sticklebacks then at large in the same pond from which the others had been taken, a servant was dispatched, who soon returned with several of them in a vessel of water. On examination, these were all found to be in the diseased state, all of them presenting the same uncouth, fiddle-shaped, appearance. They were all affected too in some degree by the same sort of globular pustules which have just been described as remarked on the dead one. Each fish had from one to three or four of these pustules, some of them on the body of the animal in different situations, and many of them were noticed on the fins, and even on the delicately webbed extremities of these members. The figure above referred to is a just, though rude, representation of one of the most tumid of these fish, and three of the little pustules in question may be observed distinctly marked on it. Having put one of these fish to death by decapitation, I proceeded to dissect it with a pair of fine pointed scissars, beginning at the anus, and cutting upwards through the bony cartilage of which the belly of the gasterosteus is composed. On laying open the cavity of the abdomen, the tæniæ were immediately discovered, not in the intestines, but immediately beneath the peritoneum. The whole cavity was so stuffed with these worms, that as the fish lay on its back, the alimentary canal and all the other intestines were completely covered and hid by them. The tæniæ appeared to lie with their heads towards one another in the centre, and having their other extremities folded or rolled up in the anterior and posterior regions of the cavity, so as to form the double protuberance so distinctly visible in the external appearance of the fish when alive. The tæniæ, which were above an inch in length, and four in number, were perfectly lively when removed from their situation. One of the globular pustules figured in the sketch was now subjected to minute dissection under the microscope, when it was found to be merely a diseased sack, formed by a distension of the skin of the fish, of the colour and spots of which it partook according to its situation on the body of the animal; having the neck which attached it to the rest of the skin extremely slender. When opened, it was found to contain a whitish coloured, and rather viscid, pus. It is probable that these pustules were merely attendant symptoms of the diseased state of the sticklebacks.

It would seem from the circumstances just detailed that as soon as the gasterosteus aculeatus has provided for the continuation of its kind, by depositing or impregnating its ova, it is immediately doomed to a gradual destruction by the tænia solida, with which it then begins to be internally infested. And if this fact be established, and the connexion between the time of spawning and that

of the commencement of the disease shall be found to be invariable, it will present one of the most curious, amongst the many, provisions of nature hitherto noticed, for preventing the too great accumulation of a particular species. I may also remark that the particular situation in which the tænia solida is found in the gasterosteus aculeatus may perhaps in some degree tend to support Dr. Chisholm's theory of the propagation of some worms hinted at in his paper on the malis dracunculus, or Guinea worm, in the 42d number of the Edinburgh Medical and Surgical Journal.

I remain, Sir, your obedient humble servant,

THOMAS LAUDER DICK.

ARTICLE VI.

Letter to Dr. Wells respecting Dew, from Professor Prevost, of Geneva.

SIR,

I HAVE but just had an opportunity of perusing your letter to Dr. Thomson, published in the April number of the *Annals of Philosophy* for 1815, which relates entirely to Dr. Young's criticism on your work on Dew. I had some time before read your letter in the November number of the same journal for 1815, in which you answer the request which I had made you for an explanation relative to the experiments of M. Benedict Prevost on the water deposited from the air.

1. On reading this last letter, and before I had seen the preceding one, I thought I saw very clearly that you considered it as impossible for water to be deposited on the outside of panes of glass when the external air is colder than the internal; but as M. Benedict Prevost affirms very positively that water is very frequently deposited under such circumstances, I immediately requested of this philosopher his proofs, conceiving myself unconnected with the dispute, because it can be decided only by experiment, theory being unable to affirm any thing, either on one side or the other.

But after reading your letter in which you discuss the criticisms of Dr. T. Young, I thought I perceived an agreement between us, not only respecting the facts, but likewise the explanation of them.

Respecting the Facts.—M. Benedict Prevost says that "dew is often deposited on the outside of glass panes when the external air is colder than the internal." Dr. Wells does not seem to deny this fact. He appears only to say "that the body on which the dew is deposited is colder than the air from which it is deposited,"*—an assertion that may be reconciled with that of M. Benedict Prevost.

* In the letter on dew in the *Annals of Philosophy* for April, Dr. Wells quotes

Respecting the Explanation.—I have said, "If water is deposited on the side in which the air is coldest, it ought to be deposited still more abundantly on that part of the glass which has been covered with a metallic plate, because that plate acts the part of a screen." Dr. Wells perhaps admits this explanation; but, in consequence of the principle which he has discovered, he ought, I conceive, to go further, and to say, "if water is deposited on the external side of the glass, we must conclude that this side is colder than the air which touches it. Yet it receives a portion of heat by conduction, through the thickness of the glass. Therefore the refrigerant cause is sufficient to cool the external surface of the glass more than it is heated by conduction." And if we ask of Dr. Wells what is this refrigerating cause? he will answer, I conceive, the *radiation of the glass.*

I shall think myself happy if I have succeeded in making my explanation agree with that of a philosopher who unites the powers of reasoning with the qualities which distinguish the good observer.

2. But I shall carry my pretensions a little further. I have observed that several philosophers of the first rank appear partial to a system opposite to that of emission. They are disposed to consider the action of caloric (and of light) as the result of undulation; and some of them, considering *cold* as equally positive with *heat*, admit frigorific rays. My reasons for being of a different opinion are founded chiefly on general physics; for in other respects the phenomena of radiation and the moveable equilibrium may be reconciled to their conceptions. This has some resemblance to the dispute about phlogiston. A great number of phenomena may be explained according to either system, merely by a change of expression. I am not, however, nor ought I to be, indifferent respecting the choice. The emission of heat, I repeat, appears to me clearly indicated, and alone conformable to the general principles of physics. But we may, I conceive, admit the opposite language: and if we do so, probably the explanations of Dr. Young would agree with mine. If this be the case (I cannot decide from the short extracts in the letter which serves me as a guide), nothing further remains than to complete it by adding the explanation which your discovery furnishes.

3. Permit me here to add a remark which supports my explanation. I have shown (in a memoir inserted in the Journal de Physique for February, 1811, vol. lxxii. p. 181) that glass in all probability transmits heat in two ways, immediately and mediately. The caloric transmitted immediately, like light, contributes obviously to the evaporation of the water which has a tendency to be deposited on the external surface of the glass. This caloric is en-

p. 249 of my Calor. Rayonn. as denying that proposition. There must be a mistake in the quotation. Nor am I aware of having any where denied a fact with which I was not concerned.

tirely intercepted by the internal metallic coating. In consequence of this there is less evaporation from the external surface of the coated glass. This partial explanation of the phenomena observed by M. Benedict Prevost is absolutely independent of the relations between the external and internal temperature and that of the glass itself.

4. My remark on the effect of clouds in preventing, or rather compensating, the radiation of heat, was certainly stated too hastily and superficially to be in any degree comparable with the fine series of observations contained in your treatise on dew. The attention which Dr. Young and yourself have paid to it constitute the whole of its value.

I earnestly hope that the re-establishment of your health will allow you to continue your useful labours. I shall be eager to profit by the new discoveries which must result from them; uniting in my sentiments of respect and esteem for men who, like yourself and Dr. Young, employ a laborious life to extend the sphere of our knowledge.

Accept, Sir, the expression of these sentiments from

Geneva, Nov. 30, 1815. E. Prevost, Professor at Geneva.

Article VII.

Correction of a Mistake in the Essay on the Relation between the Specific Gravities of Bodies in their Gaseous State and the Weights of their Atoms.

The author of the essay On the Relation between the Specific Gravities of Bodies in their Gaseous State and the Weights of their Atoms is anxious to correct an oversight which influences some of the numbers in the third table given in that essay (vol. vi. p. 328). This oversight will be found in the head or title of the third column in each table, and consists in the statement of the atom of hydrogen being composed of two volumes instead of one, upon which latter supposition the tables are actually constructed, except in the instances corrected in the third table as follows, and in a sentence in the first paragraph on p. 330, beginning "This table also exhibits," &c. which is to be expunged.

Name.	Sp. gr. hydro. being 1.	Wt. of atom, hydrogen being 1.	Wt. of atom, oxygen being 10.	Wt. of atom, oxygen being 10, from experiment.	Sp. gr. atmospheric air being 1.	Sp. gr. atmospheric air being 1, from experiment.	Wt. of 100 cub. inch. Bar. 30. Ther. 60.	Wt. of 100 cub. inch. from exper.	Elements by volume.	No. of vol. after condensation.	Elements by weight.	Observations.
Carbureted hydrogen..	8	4	5	5·09	·5555	·5555 [1]	16·999	16·999	1 hyd + ·5car	·5	1 hyd + ·5car	[1] Dr. Thomson.
Olefiant gas	14	7	8·75	8·86	·9722	·9740 [1]	29·652	29·72	1 hyd + 1 car	·5	1 hyd + 1 car	
Sulphureted hydrogen.	17	17	21·25	21·32	1·1805	1·177	36·006	35·89	1 hyd + 1 sul	1	1 hyd + 1 sul	
Muriatic acid	18·5	37	46·25	45·42	1·284	1·278	39·183	38·979	1 hyd + 1 chl	2	1 hyd + 1 chl	
Hydriodic acid	62·5	125	156·25	157·53	4·3402	4·3463 [2]	132·375	—	1 hyd + 1 iod	2	1 hyd + 1 iod	[2] Gay-Lussac.
Ammonia	8·5	17	21·25	21·5 [3]	·5902 [4]	·5900	18·003	18·000	3 hyd + 1 az	2	3 hyd + 1 az	[3] Dr. Wollaston. [4] Sir H. Davy.
Cyanogen	26	26	32 5	32·52	1·8055	1·8064 [5]	55·068	—	2 car + 1 az	1	2 car + 1 az	[5] Gay-Lussac. Ann. de Chim. Aug. 1815.
Hydro-cyanic acid....	13·5	27	33·75	33·846	·9374	·9360 [5]	28·593	—	1 cya + 1 hy	2	1 cya + 1 az	
Chloro-cyanic acid ...	31	62	77·5	—	2·1527	2·1111 [5]	65·659	—	1 cya + 1 chl	2	1 cya + 1 chl	

In this table it will be also observed that the new determinations of Gay-Lussac respecting the prussic acid, &c. are inserted, to show that they correspond with, and further corroborate, the views which have been brought forward in the essay above referred to.

There is an advantage in considering the volume of hydrogen equal to the atom, as in this case the specific gravities of most, or perhaps all, elementary substances (hydrogen being 1) will either exactly coincide with, or be some multiple of, the weights of their atoms; whereas if we make the volume of oxygen unity, the weights of the atoms of most elementary substances, except oxygen, will be double that of their specific gravities with respect to hydrogen. The assumption of the volume of hydrogen being equal to the atom will also enable us to find more readily the specific gravities of bodies in their gaseous state (either with respect to hydrogen or atmospheric air), by means of Dr. Wollaston's logometric scale.

If the views we have ventured to advance be correct, we may almost consider the πρώτη ὕλη of the ancients to be realised in hydrogen; an opinion, by the by, not altogether new. If we actually consider this to be the case, and further consider the specific gravities of bodies in their gaseous state to represent the number of volumes condensed into one; or, in other words, the number of the absolute weight of a single volume of the first matter (πρώτη ὕλη) which they contain, which is extremely probable, multiples in weight must always indicate multiples in volume, and *vice versâ*; and the specific gravities, or absolute weights of all bodies in a gaseous state, must be multiples of the specific gravity or absolute weight of the first matter (πρώτη ὕλη), because all bodies in a gaseous state which unite with one another unite with reference to their volume.

Article VIII.

On the Marquis de Chabanne's Method of Ventilating Houses. By Mr. Arnot, Surgeon.

(To Dr. Thomson.)

DEAR SIR,

Some days ago I went to examine a new plan of warming and ventilating houses exemplified at No. 1, Russel-place, Fitzroy-square, in the house of the inventor. My first impression on entering it, and beginning to consider the subject, was a feeling of surprise at its never having occurred to me before to inquire attentively whether objects of such importance are or are not completely fulfilled in our present habitations: but my surprise was much increased when a slight review of the subject convinced me that it must have been hitherto almost totally neglected by men of science.

On reflection it will be evident to all that our present houses have but a small degree either of the comfort or salubrity of which they are easily capable. In a country like ours, where so many powerful minds are familiar with the principles of mechanical and chemical philosophy, daily investigating and daily applying them, it is singular that up to the present time this intellectual energy should not have been directed to the improvement of what is so immediately important to every one of us.

Our houses, to be perfect, should enable us to maintain in them at all seasons that mild or summer warmth which agrees with us, and we should breathe the air in them as pure as it is on the mountain top. Contrary to all this, however, we find, first, as to heating, that instead of the whole house being habitable in winter, as we would desire, there are only two or three rooms so, those, viz. in which fires are kept; and in them all that is comfortably warm is but a ring of space round the fire, beyond which it is too cold, and nearer than which it is too hot. Moreover, even in this favoured ring, in place of the equable heat which our constitution naturally demands, we find a thermometer on the side of the body next the fire, standing many degrees higher than on the other, and a chilling current of air constantly tending towards the fire. In producing these miserable effects, too, we err so much against proper economy that, instead of applying the whole of the heat evolved in the combustion of our fuel to the purpose for which it is wanted and intended, we allow three-fourths of it to ascend the chimney with the smoke, and to escape perfectly useless. As to the other object mentioned, the purity of the air or ventilation—as our houses are now constructed, the only outlet by which noxious exhalation from our lungs, &c. &c. can escape, is a chimney which opens near the floor of each apartment; and yet all know that the hot air from our lungs, &c. immediately rises to the ceiling. It must descend again from thence, when cooled, to reach the chimney; but it is evident that in so doing it must be again partially inhaled, and will be again sent aloft doubly pernicious still to descend.

There are doubtless many persons in England to whom if such a statement had been made, they would readily have found the remedy; but, like many other simple applications of known principles, notwithstanding its universal importance, and probable future universal adoption, it has come later than might have been expected. The proposer of the new plans has first viewed the subject in the necessary light, and he seems to have succeeded well in his attempt at correction. The following is an outline of the plans.

1. *As to Heating.*—Instead of allowing the hot smoke of a fire to escape useless, as is now done, it is made in ascending to pass through certain pieces of apparatus called warmers, placed in different parts of the house, to which it parts with its heat; and these, by becoming warm themselves, heat the air surrounding them in the apartments, as a hot iron or stone heats water into which it is

thrown. This abstraction of the heat from the smoke is effected in a variety of ways, but generally by a warmer placed in the fireplace of each room over that in which the fire is lighted. The heating may be carried to any extent, and is regulated by the turning of a cock or valve which governs the admission of the smoke into the apparatus. In addition to this saving, the inventor also produces more heat from a given quantity of fuel, for his stove is so contrived as to burn the smoke, that is, a large quantity of carbonaceous matter which usually rises with it, and much inflammable air which usually escapes unseen.

2. *As to Ventilation.*—The vitiated air rising from the lungs, or from candles, lamps, &c. in the room, immediately passes away through a circular opening in the ceiling, from which a pipe conveys it to a large common tube in the staircase, and this last rises through the roof like a chimney, and is crowned by a ventilator. Through it the air from the upper part of every room in the house is constantly passing away, and mixing with the atmospheric ocean above, ensuring thus the absolute purity of what remains.

The advantages of these plans over common methods may be most conveniently enumerated as follows:—

1. *With a View to Economy.*—One fire answers the purpose of several, with a saving of fuel proportionate. There is not the trouble of lighting and attending to many fires, and of cleaning the stoves and fire-irons; so that in many houses a servant might be spared. In many houses fires are required to be lighted from time to time in every apartment to remove dampness, and this during summer as well as in the winter; and it is not uncommon even for families to submit to the inconvenience of migrating from room to room on this account, while the purpose is immediately answered in the new plan, by directing the kitchen smoke through the warming apparatus of any particular room. The original expense is little more than that of setting common stoves.

2. The danger of fires in houses is necessarily much diminished by it.

3. There never can be smoke in a house so warmed, nor the consequent expense of renewing so often the papering and painting on account of it.

4. If elegance be studied, the new plan is susceptible of as much embellishment as the old. As we have been universally accustomed in this country to see the fire which warms us, and to account it company in the nights of winter; for those who might be unwilling to relinquish this enjoyment in their sitting rooms at least, the inventor has planned some elegant specimens of a combination of the stove and warmer. These possess nearly the full advantage of the new plan with what is very pleasing to many of the old.

5. *Temperature or Climate.*—In every part of a house so fitted up the impression must always be that it is summer. A delightful uniform warmth is felt on all sides, quite distinct from the partial and unequal heat of a fire. There can be no pernicious draughts

of air, damps in bed-rooms, chills on coming into the staircase, or on moving from one room to another; and these are the causes of half the winter diseases of our climate. An invalid in a house so warmed and ventilated need scarcely regret the climate of Lisbon or Madeira.

The new apparatus shuts up the chimney altogether, except the little tube of the stove where a stove is used. The importance of this effect in securing uniformity of temperature may need explanation to some. In our ordinary chimney there passes up not only the air which has fed the fire, and has been heated by it, but a much larger quantity which enters it between the bottom of the mantlepiece and the fire. It is this last abstraction which produces the powerful and dangerous draughts which we always feel tending towards the fireplace, and which causes the sudden fall of temperature in a room when the door is left open for a minute. There passes off too, unfortunately, only the lower stratum of air into which the chimney opens, while the heated air above remains untouched. A common chimney, therefore, in addition to the great draught produced by it, must necessarily maintain a heated and impure atmosphere above the level of the mantlepiece, surrounding the heads of the company, and being breathed by them; and below, a stratum of cold air moving towards the fire, pure indeed, but answering only the purpose of chilling dangerously our feet and nether bodies which are immersed in it.

6. *The Purity of the Air.*—Man's existence depends immediately upon the agency of the air, of which he consumes in breathing the vital principle. Deprive him of air but for a minute, and he becomes senseless, and dies. Confine him to a small quantity without change, and the same effect as certainly follows. Change the air in any known way, or his body in its disposition to be affected by it, and the most striking results follow. With all this before us, it is singular that many of the certain consequences of breathing vitiated air should so long have been attributed rather to other causes than to the real one. While we see gaol, ship, and hospital, fevers, arising as necessary consequences where many persons breathe together in confined places, we have often attributed to want of exercise only the consumptions, debility, paleness, and premature death, of persons of sedentary habits or employments. Persons who are much abroad in the open air, and those who are not, may in all cases by their appearance be very easily distinguished from each other; and it is only among the former that we meet with longevity and vigorous health. By the plan of ventilation now proposed, it is evident that the air in a small study or bed-room can never cease to be as pure as under the open sky.

The proposer of these improvements, the Marquis de Chabannes, has received a patent, which ensures him some advantage from their adoption. His name will suggest a reflection, which has already often been made, that England has not only the honour of making the most important discoveries herself, but it is to her also that in-

ventive genius of other nations often comes to receive its reward. This Gentleman found refuge here with a numerous and infant family from the storms of the Revolution. I understand that his own estimable character, and his very interesting family, have procured for him here the attachment of many of our countrymen; and he clings to their friendship, and to English security, in preference to any thing which his former country now offers. If, besides informing the public on a subject very important to it, I by this little account also render a service to such a man, it will be an additional pleasure to me.

In considering the progressive improvements which men have made, and which at the present day they are making more rapidly than ever, in the arts of life, it is impossible not to be forcibly struck by the contrast which occasionally appears between early attempts and subsequent perfection. With regard to the subject of the present paper, for instance: man, without shelter and protection, requires for his comfortable existence the warmth of countries on which the sun darts nearly perpendicular rays, still by progressive art he has contrived to make even the regions of the frozen pole afford him almost every enjoyment of which his existence is capable. It was near the equator that Omnipotent Benevolence placed the first inhabitants of the earth, when new-born reason had not yet learned to mould obedient nature to its purposes; but as the race multiplied and spread, shelter was to be sought against the chilling blasts which were now found threatening death to the tender organizations of warmer climes. Here man built his cabin, and shut out the storm; he lighted his fire, and the noise of the elements without made him but feel more sensibly his comfort and security within. His means are now so complete, that he produces at will the climate which pleases him, in whatever part of the world he be placed; and thus while all other animals, unless protected by him, must perish when removed from the zone in which they first appeared, he, the lord of all, has made the whole earth his comfortable home, and its varieties of climate but minister to his pleasures.

I am, dear Sir, yours, &c.

Brunswick-square, Dec. 18, 1815. A. Arnott.

Article IX.

On Lighting Coal Mines. With a Plate.

Dec. 2, 1815.

Pereant qui ante nos nostra inveniunt.

The accompanying figure (Pl. XLIII. Fig. 2) of an air-tight lantern to be used in coal-mines is in principle and form unquestionably of my own original conception, invention, and construction, many months ago. Upon the production of Sir H. Davy's lantern it was

sketched, a copy taken, and, together with the following observations, awaited the judgment of the public upon that invention, as it might be declared or expressed in one or other of our two philosophical journals. In No. 36 of Thomson's *Annals* for Dec. 1815, a lantern constructed by Dr. Murray, of Edinburgh, upon the same principle as this, of supplying itself with air from the bottom of the mine, is announced as having been exhibited and put in a course of trial. Although before the 1st of December, therefore, I had never heard of, and have as yet never seen, any representation of Dr. M.'s lantern, yet to him, repeating without any malignity of imprecation, the prefixed motto, amended from the adage "pereant qui ante nos nostra *discerint*," I resign the honour of prior discovery, and, which is of more importance, of future practical application.

The figure of the lantern shows its use. It receives the supply of air with which it burns exclusively from the bottom of the pit, through the flexible tube of leather, covering a spiral wire, and terminated by the perforated globe of metal at its lower extremity, which may drag along the ground, whilst the miner carries the lantern, or lie stationary upon it, when the light is stationary. This invention was suggested by the following observations, which may not be undeserving of notice, although the lantern of Dr. M. precede in existence, and be preferred in use.

Coal-mines are infested with two sorts of noxious airs, differing essentially from each other in all their properties. The one, called by the miners the choak-damp, the azote and carbonic acid gases of chemical philosophy, is heavier than atmospheric air; the other, called fire-damp by the miners, the carbureted hydrogen gas of chemists, is lighter than atmospheric air. Of course the places occupied by each are the bottom and the top, the floors and roofs, of mines. Of these gases the former become less and less noxious in proportion to their commixture with atmospheric air; the latter more and more dangerous, and liable to explosion, in proportion to the same commixture, in quantities limited to six parts and 12 parts of atmospheric air. No commixture of these different noxious gases will explode.

These various properties of these gases indicate the modes to be pursued to discharge them from mines, and to destroy their noxious qualities. The light air can be, and is, fired with safety, and consumed as it issues from the crevices of the mine before it mixes itself with the atmospheric air in proportions capable of exploding. But beyond all question the best and most direct mode of getting rid of it is to conduct it with as little agitation as possible, and mixture with the air of the mine, along the roofs of the mine or channels of intersection cut therein, to which in the original workings of the mine, and at all times afterwards, a due degree of inclination should be given for the purpose, to conduct it to up-air shafts, at which it would regularly and safely be discharged. Wherever the workings of the mine by irregularity of rise or elevation of roof

should render it impossible to form these channels of connexion with established air shafts, a new air shaft should be formed to command the upward air-drainage of the new workings. Might not this be effected, and the number of up-air shafts be considerably increased without exceptionally deforming the upper surface of the soil, by ascertaining above by trigonometrical calculation the point from which descending a new shaft might be established; and might not such shafts be formed by an improved process in boring and letting down iron pipes to preserve and to keep open the shaft? If, in boring, any considerable quantity of water should be pierced, more than can easily be disposed of (the great objection to air shafts), as the boring would be from the top of the surface, no harm would ensue, and the boring on that point might be abandoned.

The heavy, or azotic and carbonic acid gases, can only be rendered innoxious by ventilation and mixture with atmospheric air introduced from above by currents established by mechanical means through shafts and channels passing to the very bottom of the mine, or by giving directions to the currents produced by various causes within the mine itself.

These mechanical means of discharge are applicable to these gases as noxious to human life generally, and undoubtedly they are the great and direct means to be resorted to for these purposes. The insufficiency of these means always to discharge the light gas, its explosibility, the necessity of using fires to give light in the dark abysses of the earth, and the difficulty of ascertaining when and where the dangerous accumulations thereof exist, have rendered it a desirable thing to discover, if possible, any mechanical contrivance by which the benefit of light may be obtained without the danger of fire.

Two of the plans recommended for the purpose will be considered: that which in times past has been invented, and continues to be now used, of obtaining light by the collision of flint and steel; and that which in the present day has been proposed by Sir Humphry Davy, and is now before the public, of a closed lantern, the passage of air through which is duly regulated.

The use of flint and steel does not seem to be sufficiently understood in its principles. That it is not secure against explosions is admitted; and I am inclined to think that the dependance upon, and safety expected from, its use, is in a great measure illusory. Whenever it has produced explosion, it is not doubted that the flame of a taper would have produced it. Would the flame of a taper produce it where the collision does not? I am inclined to think that the light in fire, for such it is, produced by these collisions, fails to fire air not explodible, and fails not to fire explodible air.

If the collision of flint and steel be made *in vacuo* at the points of contact, and there only, light is exhibited; when the collision is made in atmospheric air, the abraded portions of steel fly off in a high state

of temperature, produced by the contact, and absorbing oxygen from the air, suffer combustion, and an exaltation of temperature even to white heat. If, therefore, the collision be made in atmospheric air, combustion of the steel takes place. If in pure hydrogen gas neither combustion of the steel, nor explosion of the gas, takes place, for want of a due commixture of oxygen. If the collision be made in a due mixture of oxygen and hydrogen gases, will not both combustion of the steel first, and consequent explosion of the gas, be produced? Perfect safety is not, therefore, to be expected from the use of these instruments, but this developement of the danger in its principles leads to this practical improvement in the machine. The chance of danger may be diminished by a construction that shall subject the steel only to close contact without any separation of parts, leaving all abrasions to be of the parts of flint which are incapable of further exaltation of temperature.

Sir H. Davy's lantern diminishing the supply of atmospheric air by adjustment of the apertures through which alone it enters and passes out, diminishes the flame. Admit hydrogen gas pure or not sufficiently mixed with atmospheric air, to explode; the pure gas will burn without explosion, as it does in the lamps of the metropolis, and goes out, as it would in those lamps if the supply of fresh air were excluded from all other entrance, and by the gas itself where it enters. A non-explodible mixture of gases burns in the lantern, as it does at the taper of the miner, who, when he sees the flame *capped* with a surrounding flame, knows his danger, gently depresses his candle into a lower stratum of air, and retires. The lantern does nothing for him which his own observation of his candle does not; and if the flame of the lamp will thus fire this non-explodible mixture of the gases, what is there to prevent the inflammation and explosion of explodible mixtures, and the communication of the flame through the apertures of the lantern with that body of mixed gases which is external and adjacent? The lantern, therefore, will not explode non-explodible gases, and will not fail to explode explodible gases.

Various other considerations present themselves in opposition to the use of this lantern. The flame, it is supposed, renders, as it burns, the air in the lantern less fit for combustion, by portions of azote and carbonic acid gases which mix therewith. If in the first minute a given portion of these airs be mixed with the air in the lantern; in the second minute, another; and in the third, and other succeeding minutes, successive rateable portions; the power of combustion must end, and the flame be gradually extinguished. This difficulty can only be surmounted by an adjustment of apertures, which supposes or renders this successive deoxygenation subject to certain limits of existence? If from this supposed period we reason back, will not the agency of the same causes which at any time prevent, always prevent, any deoxygenation, even at the commencement of the inflammation; and does not all this prove that the reduced state of the flame observed in the lantern depends upon some other cause rather than the quality of the air?

If the reduced state of the flame depends, as is supposed, upon the quality of the air; and if in atmospheric air the adjustment of apertures be such as to prevent the spontaneous extinction of the flame therein, other and various adjustments will be required for the various mixtures entering the lantern of atmospheric air and carbureted hydrogen gas, to prevent the successive extinctions of the light in the absence of all danger; and will not this require a re-opening of the lantern to variously mixed airs, that will abate and reduce again the safety of the miner to open lamps?

As the success of this invention is stated to depend primarily upon commixture with the supposed foul air within the lantern of entering explodible gases before they reach the flame, this may best be effected by many small holes in the floor of the lamp at greatest distances from the flame. But is the air within the lantern which is said to reduce the flame, and which is considered capable by commixture of preventing explosions, as foul in fact as it is supposed to be? No azote or carbonic acid gas from the combustion of the lamp can remain or will be found within the lantern. Their specific gravities in their heated state at the moment of combustion must be considerably less than that of atmospheric air, and therefore they will first rise through the chimney, and will rise urged upwards in a current that would carry them out of the lantern, even if heavier than atmospheric air. The reduction of the flame in the lantern, which is attributed to, and is supposed to be, evidence of quality, is in fact owing to the smaller quantity of air which has access to the flame in a current whose rapidity is diminished by the delayed escape, through the diminished chimney, of the heated azote and carbonic acid gases. The escape of these airs is retarded, not prevented; they must go before any other air. By their retardation, the current of air which supplies the flame is retarded, and the flame diminished in consequence of this diminished supply, but none of the azote or carbonic acid gases will remain to mix with the air in the lantern, to affect the flame, or to prevent explosions, as is supposed. And if explosion takes place within the lantern, will small or large holes in themselves, or elongated into tubes, prevent explosions of the gases from passing in train through them, and communicating with the external explodible mixtures from which they proceed? This cannot be conceived of any explodible mixture continued through channels of uninterrupted communication of any dimensions. Yet is this suggested, and explosions are said to be "incapable of passing through small glass or metallic air tubes." In the *Annals of Philosophy* this possible danger through the apertures is suggested. In the Philosophical Magazine for the first time air tubes of supply were contemplated. The experiment should be made. It would seem that a discharge in train is to be expected through tubes of all lengths and dimensions, from the touch-hole of a fusil to all greater lengths and bores.

Contrary, therefore, to what has been reasoned respecting this lantern, the flame is not primarily reduced by change of quality in

the air, but principally by change of quantity, from the diminished supply of the retarded current. The flame is extinguished principally in consequence of atmospheric air being excluded from all access to it, by the entering gases. The flame is not extinguished until the gases arrive at and are fired by it; and if they be explodible, they will explode, and communicate with the gases without.

The only remaining plan indicated is that conformably to which the accompanying lantern has been constructed, to exclude all communication of the flame from the air within which the lantern is placed, and to derive its supply of air from the floor of the mine, from that stratum into which the alarmed miner depresses his taper, which if it be of azote and carbonic acid gases, or choak-damp, will only extinguish his light, and only in the last supposeable extremity of a mine filled to the bottom with fire-damp, will produce explosions, could any person be supposed capable of existing in such a mixture of carbureted hydrogen gas, or of persisting to advance into it, notwithstanding the various other notices he would have received of his danger.

This evil results from the consideration of these expedients. Up-air shafts are neglected, and a due course of mining, whilst the result is awaited of contrivances which, after all, can only be auxiliary to those, and are injurious, as they tend to supersede their use, and prevent their establishment. In the west I understand that collieries are opened and conducted upon principles which discharge all accumulations of light gas by upward drainage, and prevent all descent of waters below their drainage level from the surface of the earth. Conformably to these principles, as far as may be, the mines in the north should be improved, opened, and conducted in all their future workings.

Article X.

An Essay on the Shapes, Dimensions, and Positions of the Spaces in the Earth which are called Rents, and the Arrangement of the Matter in them: with the Definition and Cause of Stratification. By Mr. John B. Longmire.

(*Concluded from* vol. vi. p. 414.)

On the Cause of Formations.

I have already gone through, in a brief way, those parts of my advertisement which relates to internal and surface rents, and to stratification; and I have now only to show that the phenomenon of formations is an "effect of the unequal contraction of the earth's matter."

In this essay I will adopt an arrangement, as consistent as I am able to make it, with the manner in which I conceive the visible

part of the earth has been formed. I shall not bring forward all the observations necessary in a complete system, but only such as are most intimately connected with the proposition under consideration; and I shall in general, and as much as I can, confine myself to new data; observing, in the mean time, that all the old and well established facts fall in with these data in such a way as to lead as near as possible to a true theory.

Some geologists arrange all the known matter of the earth under the classes primitive and secondary; others make the divisions primary and secondary; and others divide into primitive, transition, and floetz. None of these classifications is sufficiently correct. The term primitive probably originated from the idea that the matter so called is in its original solid state; and if this word were not applicable to matter arranged in the other classes, and had no reference, in contradistinction, to the term secondary, it would certainly in this sense be applied with propriety; but when, considered with reference to the word secondary, its aptness is altogether lost; for though secondary matter is not in its original situation, it undoubtedly possesses its first state of solidity. In other respects these terms are improper: the greatest part of primitive matter is unstratified, but some varieties are stratified, as, for instance, quartzy sand-stone, or stratified quartz, compact green-stone, &c. Now as primitive matter is both stratified and unstratified, the distinction is lost, and the matter of two classes confounded together. These objections apply with equal force to that arrangement which divides the earth's matter into the primitive, transition, and floetz classes. To these objections may be urged those which follow. All the matter belonging to the classes primitive and transition is strictly primitive; therefore if a distinction be made in classing this matter, it ought only to be of subordinate consideration. If the term floetz literally means stratified, it is sufficiently distinct, provided no matter is arranged under it that is not stratified; at any rate, however, stratified is by much the better term. But whatever be the opinions of geologists as to the earth's mode of formation, two terms, namely, *concrete* and concrete-stratified, may be used, which accord exactly with its structure, and which have very little reference to theoretical speculations.

Of Concrete Matter.

The concrete matter comprehends the primitive or primary matter; and includes all the primitive; some, if not all, of the transition; and a few of the floetz, and the newest floetz trap formations of Werner. This matter may be divided into earth-stone and lime-stone.

1. *Of Concrete Earth-stone.*

The earth-stone completely surrounds the inner part of the earth. It is a concrete mass that can only be distinguished into

parts which differ from one another in appearance; and the *essentially differing parts* are arranged into *completely concentric layers.*

That the concrete earthy matter is a great and universally continuous mass, I will now endeavour to show. The visible parts of this matter are generally situated on the highest parts of the dry land; on a lower level, the concrete lime-stone puts on; and below it the stratified matter commences. Were we to draw a sectional line through a large visible part of this matter, in any direction except the longitudinal one, the centre of the part would be the highest point, and from this place the line would bend downwards in a regular and easy curve, till either the lime-stone or the stratified formations were met with; and if the former, the line would still descend, but easier than before, till it reached the stratified formations; it would then incline a little, but would be nearly horizontal: if we pursued this line straight forward, it would ascend easily the contrary way, while it passed through the stratified ground; then steeper over the concreted lime-stone; and still steeper over the concrete earthy matter, to the summit of the ground. After observing this line again descend for a certain distance, it would re-ascend; thus passing over a visible portion of earth-stone; then over the lime-stone, or the stratified formations, or both; and then over another part of the earth-stone; and so on alternately. When we observe the curve of the earth-stone, and examine how the lime-stone and stratified matter put on, we at once give the negative to the idea that the earth-stone terminates where the lime-stone or stratified matter commences, and conclude that it continues under them; and as a proof that this conclusion is just, we actually find some of the earth-stone's great eminences reaching above them. Again, when we have traced the descending part of a series of stratified formations, and meet with another elevated part of the earth-stone, it appears as if it rose from under the stratified formations near it; in other words, its surface inclines to the other portion in such a way as to show that these portions are only the visible parts of *the same mass.* Hence we may infer that the earth-stone is an universally continuous mass that exists every where in low as well as in high situations; of course it envelopes the inner part of the earth, and contains in its surface-hollows the lime-stone and stratified matter.

The earth-stone is divisible into parts which differ in external characters. Such parts, however, are firmly united together. The parts that differ essentially from one another are arranged in concentric layers. There are two layers exposed at the surface, and found in mines. The lower layer is granulated, or grained, and consists of granite, sienite, &c.; the upper layer is compact, and is composed of clay-slate, mica-slate, &c. The concentric arrangement of these layers will be evident from what follows. In every extensive part of the earth-stone we find at least one variety of grained earth-stone. Now if we make observations on the shape

and relative situation of such parts of the grained earth-stone, it becomes evident that they are the visible parts of a concentric layer which underlies, but is united to, the compact layers. Let us instance the primitive district in Cornwall and Devonshire. If a person views the section of the largest granite hill near Redruth, taken in a straight line towards Dartmoor, in this direction the surface of the granite descends regularly till covered with the compact layer; then this layer descends also in the same direction, for a certain distance, but not so steep as the grained layer, or granite. Let this person now remove himself to Dartmoor, and take a sectional view of this hill in a straight line to the granitic hill near Redruth. The Dartmoor granite descends towards Redruth precisely in the same way as the Redruth granite does to Dartmoor, till it is covered by the compact layer; which also declines to the place where it dips to from Redruth. Now the inference is, that these are two of the highest parts of a granitic layer which lies totally under the compact layer in this part of the country; and the fact is, that the granite and granitic veins, which proceed from it, are found in the intermediate space between Redruth and Dartmoor. The same conclusions may be drawn if a person looks towards the granite near the Land's End, or towards the Bristol or British Channels. Hence these granitic masses are but the visible parts of a grained layer that lies under the compact layer in this part of the world. Now as all granite or other grained parts present the same appearance as these, and exist in every elevated part of the earth-stone, the grained layer which is found in Cornwall is but a part of such a layer that continues in every direction round the world, and appears in every country, in consequence of having an undulating surface, whose eminences reach above the compact layer. Finally, specimens of the different varieties of grained earth-stone, such as granite and sienite, approach so near one another in appearance, that it requires the most expert mineralogist to determine with precision to which variety some of them belong. There, therefore, appears to be no difficulty in the way in supposing that granite and sienite are portions of the same mass; on the contrary, this deduction might have been drawn from their approximating appearances.

The undulated surface of the earth-stone was produced in the following manner. The process of consolidation proceeded upwards from below till it reached the earth's surface. Before it had acted on the grained layer, a number of inequalities commenced in the surface, which divided the solid from the fluid matter, because the matter below contracted more, and in consequence sunk lower in one place than in another; as the consolidation continued upwards, these inequalities increased, and at the surface gave rise to the undulations which I have described, when speaking of the earth's features, as being peculiar to primitive districts, and also produced such undulations as contain a series of stratified formations in their lower parts. The hollow between the top of Keswick Mountains,

and the top of those in the Isle of Man, is the lower part of one of the last-mentioned undulations. There would be a time when the undulations in the surface between the solid and fluid matter were so great, that the latter matter would retire into hollows, and stand just as high as the highest parts of the former matter. In the continuation of the process, the elevated parts of the solid earth-stone would gradually extend above the surface of the earth-stone's fluid matter; because the *hollows* in the surface of the solid matter *would increase in size*, and the still fluid matter retire into them. In this way the fluid portions of the compact layer got below the higher parts of the grained layer; and till the consolidating process reached the general surface of the compact layer, the fluid matter kept sinking, and its surface retiring downwards, and leaving a part of this layer solid and united to the grained layer; but when nearly the whole of the compact layer had obtained the solid form, its surface assumed throughout a series of small undulations, whose dimensions continued to increase till the matter had reached its present degree of solidity.

2. *Of Concrete Lime-stone.*

This matter comprehends all those grained and compact lime-stones and chalk which are not stratified; and includes most of the primitive, transition, and floetz, lime-stone and chalk, formations of Werner.

The matter of the lime-stone while in a fluid state completely covered the earth-stone. When the compact layer had assumed the solid form, the consolidation of the lime-stone layer commenced. But previous to this event the fluid calcareous matter had first sunk gradually below the tops of the grained layer, and then below the higher parts of the compact layer. As the inequalities in the surface of the earth-stone are very great, the universal continuity of the lime-stone was destroyed, and parts of it, while still fluid, were kept back in hollows; and parts arrested by the consolidating process as they were retiring downwards. It is, therefore, found either bending round hollows of the earth-stone, or only lying in parts on one of the sides of such hollows. We are now come nearly to the conclusion. Had there been no inequality in the contraction of the earth's matter after it had assumed the solid state, the grained and compact earth-stone layers, and the lime-stone layer, would have been completely hid by a stratum of water equal in quantity to the present ocean, which last may be considered the remains of that fluid from which the lime-stone proceeded; by the same rule the stratified formations could not have existed. But as the unequal sinking produced hollows, and exposed the earth-stone and lime-stone to view, and as the ocean retired into, and was again forced out of, these hollows to its present situation, by the slow and progressive accumulation of stratified formations, this unequal sinking of course gave rise to a variety of formations. Now

as the unequal sinking is a consequence of the unequal contraction, the phenomenon of formations must be "a consequence of the unequal contraction of the earth's matter."

Having now finished what I proposed, I will close these essays with some general remarks on stratified formations. I consider the great formations, namely, the sand-stones and coal formations, to be formed in the manner that lakes are at present filling up. The sand-stones are *large bars* deposited by former rivers at the places where they entered former lakes, and the coal formations are the *sediments* of these lakes.

I scarcely need remark how well the position, relative situation, and the nature of these formations, accord with this idea; nor need I show with what facility this theory at large accounts for the existence of entombed marine animals so high above the present ocean; nor why these bodies are in general confined to lime-stone, the large vegetables to the sand-stone, and the small to the coal formations; as these phenomena admit of the easiest and clearest explanation upon the principles displayed in these essays.

Article XI.

Extract of a Letter from Mr. Benedict Prevost, Professor at Montauban, to Mr. Pierre Prevost, Professor at Geneva, respecting the Dew which is deposited on that Side of Panes of Glass where the Air is coldest.

(To Dr. Thomson.)

SIR,

The annexed abstract of a letter from Mr. B. Prevost relates to a fact which has been discussed in your Journal (vol. vi. p. 379, 432).* I think your readers will consider it as necessary to complete the discussion in question.

I am, &c.

(Signed) P. Prevost.

Geneva, Jan. 1816.

As to the humidity which is deposited on the outside of a glass pane, though the air on the outside be colder than that within, I am certain that I have observed it several times, pretty frequently indeed. I mean that I have observed this humidity on the outside when the external thermometer was lower than the internal. But you know, my dear cousin, and you state it yourself (at No. 24 of your § 192, p. 241), that I endeavoured to procure thermometers with flattened bulbs, and sufficiently sensible to point out the diffe-

* See also the present Number, p.

rence of temperature of the sides of the glass at the same time, which I think would have given me instructive results; but I was not fortunate enough to succeed.

I mark down with regularity, in a journal containing the height of the internal and external thermometer, the dryness or moisture of the glass. I find in this journal several confirmations of what I have said. I even find some days in which the glass was wet without and moist within.

It may be objected to me that these observations prove only the co-existence of this external humidity, and an external temperature lower than the internal; and not that this external humidity was deposited while the external temperature was lower. I must acknowledge that I do not recollect to have seen the moisture depositing itself on the glass, while the external temperature was lower than the internal; and I cannot at this moment, for reasons that I shall mention to you afterwards, consult the original journal of my experiments. But the following observation, which I have an opportunity of repeating every year, does not appear to me to agree with the empyrical principle established by Dr. Wells, "that bodies are not covered with dew unless they be colder than the air." The window shutters of the country house in which my experiments on dew are made are painted green with oil paint. When open, they are applied (at least in part) to the wall of the house. Now in the cold season, when the nights are fine, they are often so covered with moisture as to be dropping with water in the morning, while the air has been getting colder during the whole time, as is shown by thermometers fixed against them. This happens principally when the preceding day has been fine, especially if it has been hot. But as in this case the external air deposits moisture on these bodies while cooling, it follows that dew (or humidity) is deposited on bodies hotter than the air which surrounds them, and which gives out this moisture.

It will be said, perhaps, that it is the dry and cold air of the higher parts of the atmosphere which, descending during the night, produces the cold, while the humidity is deposited from the hot and moist air which ascends. But upon the whole the air which descends uniting with the ascending air must always cool it. That air accordingly is colder, &c.

My chamber looks towards the north, and the roof of the house projects some feet over the building, so that in winter the ground at the bottom of the wall is in the shade. Though I have a fire in the room during the day, and often till pretty late at night, if in the evening I shut the shutters, though I leave the window open, still a great deal of moisture is deposited on the outside in circumstances similar in other respects to those mentioned above. Here the external air appears to be colder than the window shutter, and yet it deposits a good deal of humidity on it. Yet, as I have already observed, all the ironwork of the shutter, whether painted or not, open or shut (with the exception of some pendent or very

salient parts, not painted, and covered with black oxide, as iron usually is) remains absolutely dry. Among other parts, the heads of nails, whether higher than the surface of the wood, or sunk below it.

I may observe, likewise, that the water deposited on the shutters during the night is often exhaled in the morning in the midst of a thick fog.

Perhaps in explaining these phenomena we ought to consider separately the effect of the cold air which descends, and of the hot vapour which ascends. The first probably cools the surface of the shutter only to a very minute depth, in consequence of its inconductibility, so that its temperature is lower than that of the vapour which ascends, although the air of the room and the inside of the shutters be hotter than this vapour. This seems to be confirmed by the dryness of the ironwork. This, I conceive, would make my observations agree with those of Dr. Wells. In that case, both of us ought to modify a little our general formula. The air has often less to do with these phenomena than would appear at first sight. As there is probably no action between the molecules of air and those of vapour (except what those of air may produce on each other, and those of vapour on each other; and philosophers, I believe, are sufficiently agreed on the subject), while no chemical combination takes place, it is possible that the particles of aqueous vapour distributed through the cold air may preserve a higher temperature for a time long enough to rise to a certain height; so that the air in the neighbourhood of the window shutter, though colder than it, may notwithstanding contain a hotter vapour, &c.

I here subjoin some observations found in my meteorological journal after the above was written, by inspecting it more carefully.

	Hour.	Exter. Therm.	Inter. Therm.	
1813. Nov. 25, Evening..	6° 30′	8·3°	8·1	Almost calm.
	8 10	7	8·3	G. W. E. Fine. Almost calm.
	11 0	6·1	8·1	G. M. E. Fog. Some stars seen. Almost calm.
Dec. 28, Evening..	6 45	1·7	4·6	G. W. E. Fine. Some light clouds. Slight fog. Calm.
	7 40	1·5	3·8*	G. W. E. Very fine. Slight fog. Calm.
Dec. 29, Morning..	8 0	—	4·1	
Evening..	0 40	—	4·2	
	6 15	—	4·2	
Dec. 30, Morning..	6 45	—	4	
Evening..	3 0	—	4·8	
	5 3	—	4·6	
Dec. 31, Morning..	8 25	—	4	
Evening..	3 0	—	4·3	

* The inside thermometer is suspended against the glass itself.

	Hour.	Exter. Therm.	Inter. Therm.	
1813. Dec. 31, Evening..	5° 40′	2·8°	4·1	G. W. E. Very fine. A slight fog. Hoar frost. Wind S. S. W. slight.
	8 0	1·5	4·1	G. M. E. and G. M. I. Very fine. Very thin clouds at the horizon. Very slight fog. Very slight wind N. W.
1814. Jan. 1, Morning ..	5 30	0·5	4	A thick fog.
	9 15	0·6	3·9	G. M. I. Slight fog. Fine above. Hoar frost.
Evening ..	9 0	0·6	3·9	G. M. I. and G. M. E. More moist externally than internally. Fog. Little wind.
	9 50	0·3	3·8	G. M. I. and G. M. E. Slight fog. Very fine. Calm.
Jan. 2, Morning ..	11 40	2·6	3·7	
Evening ..	2 40	3·8	3·7	
	6 40	1·8	3·8	G. R. E. Very fine. Scarce perceptible fog. Calm.
	9 0	0·7	3·7	G. M. E. and G. W. I. Very fine. Scarce perceptible fog. Calm.

N. B. All these observations were made at Montauban. The window looked towards the west; and the external thermometer fixed in the middle of the window was turned to the north.

G. M. I. signifies *glass moistened internally.*

G. R. I., *glass running down with moisture internally.*

G. W. I., *glass wetted internally.*

G. M. E., G. R. E., G. W. E., *the same externally.*

Article XII.

Analyses of Books.

Philosophical Transactions for 1815, *Part II.*

Our account of this volume will be shorter than usual, as the greater number of the papers which it contains have been noticed in the Account of the Improvements in Physical Science, which occupies the larger part of the last number of the *Annals of Philosophy*. It will be necessary, therefore, in most cases, merely to refer to the page in our last number containing the abridgment of the paper in question.

This part contains the 15 following papers:—

1. *On some Phenomena of Colours exhibited by thin Plates.* By John Knox, Esq. (See *Annals of Philosophy*, vol. vii. p. 8.)

2. *Some further Observations on the Current that often prevails to*

the westward of the Scilly Islands. By James Rennell, Esq. F.R.S. —In the year 1793 Major Rennell published a paper in the Philosophical Transactions pointing out the existence of a north-westerly current setting in between Ushant and the Scilly Islands. In the present paper he states further proofs of the existence of this current. He supposes it to be caused by the prevalence of westerly winds, which occasion an easterly current towards Cape Finisterre and Cape Ortegal. This current proceeds along the northern coast of Spain, and assumes a northerly direction when it comes to the coast of France. In consequence of the north-westerly direction of the west coast of France, the current assumes the same direction, and accordingly proceeds from the Saintes and Ushant to the Scilly Islands. The new proofs contained in this paper are the following: 1. The Earl Cornwallis Indiaman, in 1791, being 53 leagues west of Cape Finisterre, experienced an easterly current amounting to 26 miles in 24 hours. 2. A bottle thrown out of a Danish ship situated a little to the north of the Earl Cornwallis was drifted ashore at Cape Ortegal. 3. Admiral Knight, off Cape Ortegal, found the current E. S. E., or nearly along shore, and at the rate of one mile per hour. 4. Admiral Payne being off the Saintes in a severe S.W. gale, was drifted 70 miles north-west. 5. Off Scilly the flood tide runs nine hours northward, but the ebb in the opposite direction only three hours. 6. Joshua Kelly, in his treatise on Navigation, published in 1733, states that an experienced Captain of a West Indiaman being in latitude 48° 30′, and approaching the British Channel, was becalmed for 48 hours, during which he was driven to the northward 46 miles. 7. On the west coast of France the mud is all collected on the north side of the Garonne, &c. and none of it is to be found on the soush side.

Major Rennel conceives that a current runs also north along the west coast of Ireland, and after passing the north coast of that island assumes a southerly direction, and proceeds at least further south than Dublin. There is likewise a northerly current along the west coast of Scotland, which proceeds along the north coast, and, assuming a southerly direction, proceeds along the east coast as far as Harwich, where it mixes with the easterly current that flows in the English Channel, and proceeds along the coast of Holland and Jutland to the Naze of Norway.

3. *Some Experiments on a solid Compound of Iodine and Oxygen.* By Sir H. Davy, LL.D. F.R.S.—(See *Annals of Philosophy*, vol. vii. p. 30.)

4. *On the Action of Acids on the Salts usually called Hyper-oxymuriates, and on the Gases produced from them.* By Sir H. Davy, LL.D. F.R.S.—(See *Annals of Philosophy*, vol. vii. p. 28.)

5. *Further analytical Experiments relative to the Constitution of the Prussic, of the Ferrureted Chyazic, and of the Sulphureted Chyazic Acids, and to that of their Salts, together with the Application of the Atomic Theory to the analyses of those Bodies.* By Robert Porrett, jun. Esq.—Mr. Porrett's analysis of prussic acid

appears to have been made with great care, and with sufficient precision. He rated the quantity of hydrogen too high, because he was ignorant of the true nature of *cyanodide of mercury*. When his numbers are corrected, by attending to the true nature of that body, his results will come sufficiently near those of Gay-Lussac. The sulphureted and ferrureted chyazic acids appear to be distinct substances, and probably Mr. Porrett's views of their constitution are correct. It is obvious that Gay-Lussac formed sulphureted chyazic acid by mixing together cyanogen and sulphureted hydrogen, though he himself was not aware of what he had done.

6. *On the Nature and Combination of a newly discovered Vegetable Acid, with Observations on Malic Acid, and Suggestions on the State in which Acids may have previously existed in Vegetables.* By M. Donovan, Esq.—(See *Annals of Philosophy*, vol. vii. p. 37.)

7. *On the Structure of the Organs of Respiration in Animals which appear to hold an intermediate Place between those of the Class Pisces and the Class Vermes, and in two Genera of the last-mentioned Class.* By Sir Everard Home, Bart. V.P.R.S.—(See *Annals of Philosophy*, vol. vii. p. 69.)

8. *On the Mode of Generation of the Lamprey and Myxine.* By Sir Everard Home, Bart. V.P.R.S.—(See *Annals of Philosophy*, vol. vii. p. 69.)

9. *On the Multiplication of Images, and the Colours which accompany them, in some Specimens of Calcareous Spar.* By David Brewster, LL.D. F.R.S. L. and E.—(See *Annals of Philosophy*, vol. vii. p. 8.)

10. *A Series of Observations of the Satellites of the Georgian Planet, including a Passage through the Node of their Orbits; with an introductory Account of the telescopic Apparatus that has been used on this Occasion, and a final Exposition of some calculated Particulars deduced from the Observations.* By Wm. Herschel, LL.D. F.R.S.—(See *Annals of Philosophy*, vol. vii. p. 2.)

11. *An Account of some Experiments with a large Voltaic Battery.* By I. G. Children, Esq. F.R.S.—(See *Annals of Philosophy*, vol. vii. p. 11.) I noticed a mistake in one of the experiments as related in Mr. Children's paper. I have since received a letter from that Gentleman, in which he has had the goodness to give me the correction of the error, which had crept in during the hurry of transcribing. The diameter of the platinum wire, of which eight feet six inches were fused, ought to have been stated, not 0·44 inch, but 0·044 inch.

12. *On the Dispersive Power of the Atmosphere, and its Effect on Astronomical Observations.* By Stephen Lee, Clerk and Librarian to the Royal Society.—(See *Annals of Philosophy*, vol. vii. p. 2.)

13. *Determination of the North Polar Distances and proper Motion of Thirty Fixed Stars.* By John Pond, Esq. Astronomer Royal, F.R.S.—(See *Annals of Philosophy*, vol. vii. p. 2.)

14. *An Essay towards the Calculus of Functions.* By C. Babbage, Esq.—(See *Annals of Philosophy*, vol. vii. p. 1.)

15. *Some additional Experiments and Observations on the Relation which subsists between the Nervous and Sanguiferous Systems.* By A. P. Wilson Philip, Physician in Worcester.—(See *Annals of Philosophy*, vol. vii. p. 69.)

Article XIII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday, Nov. 30, the day of the anniversary meeting of the Society for the election of office-bearers, the Rumford medal was given to Dr. Brewster, for his papers published in the Transactions. The following office bearers were elected for the ensuing year :—

President—The Right Hon. Sir Joseph Banks, Bart. K. B.
Secretaries—Wm. Hyde Wollaston, M. D.
Taylor Combe, Esq. M. A.
Treasurer—Samuel Lysons, Esq.

OF THE OLD COUNCIL.

Right Hon. Sir Joseph Banks, Bart.
Sir Charles Blagden.
Samuel Goodenough, Lord Bishop of Carlisle, V. P.
Taylor Combe, Esq. Sec. M. A.
Davies Giddy, Esq. M. P.
Sir Everard Home, Bart. V. P.
Samuel Lysons, Esq. Treasurer, V. P.
George Earl of Morton, K. T. V. P.
John Pond, Esq. Astronomer Royal.
Wm. Hyde Wollaston, M. D.
Thomas Young, M. D. Sec. For. Corresp.

OF THE NEW COUNCIL.

John Barrow, Esq.
Mark Beaufoy, Esq.
Henry Brown, Esq.
Sir Humphry Davy.
Philip Earl of Hardwicke, K. G.
Edward Howard, Esq.
John Latham, M. D. Pres. Coll. Phys.
Thomas James Mathias, Esq.
Sir John Nicol, M. P.
George Earl of Winchelsea. K. G.

The deaths since last anniversary, including two foreign members, have been 23; the elections about 30. The number of the Society at present is 594, to which must be added 45 foreign members, making a total of 639 members.

On Thursday, Dec. 7, a paper by Dr. Reid Clanny was read, giving a further account of his lamp for the security of colliers against the fire-damp. He has now constructed it of such a size that it may be put into the great coat pocket. It may be made of copper for 1*l.* 14*s.*, and of block tin for 17*s.* A piece of mechanism at a low price is attached to the bellows, capable of supplying the lamp with air for an hour. Dr. Clanny relates a set of trials made in an apartment filled with carbureted hydrogen gas to the exploding point, and in a coal-mine the air of which was in the same state. In both cases the air within the lamp exploded, and the lamp was extinguished, but the external air was not in the least affected.

He showed by a set of experiments that by attending to the proper mode of supplying the lamp with air, the candle will continue to burn even when the carbureted hydrogen within the lamp explodes. Dr. Clanny states in this paper that the expense for steel mills in many collieries is much greater than would be requisite to light the mine by means of his lamp. In one mine he says it amounts to 30*l.* a week. Dr. Clanny likewise gave an account of the numerous explosions that have taken place in the neighbourhood of Newcastle, and of the opposition which he has encountered in attempting to introduce his lamp into the coal-mines in the district in which he resides.

On Thursday, Dec. 15, a paper by Mr. Herschel on the functions of exponential quantities was announced; but, from the nature of the subject, could not be read.

At the same meeting part of a paper by Dr. Brewster on the properties of heat as modifying the nature of glass was read. He showed that, by heat, plates of glass acquire the properties of all the different kinds of crystallized bodies. One portion depolarizes the ray of light in the same manner as those crystals which attract the extraordinary ray towards the axis; another part in the same manner as those crystals which repel the extraordinary ray from the axis.

On Thursday, Dec. 21, Dr. Brewster's paper was continued. A great number of curious facts were detailed; but from the nature of the paper, and the constant reference to figures, it is scarcely possible to form an accurate idea of it merely from hearing it read. He found that by heating glass red hot, and cooling it upon cold iron, it acquired a permanently crystallized texture. Of all the minerals tried, obsidian was the only one whose texture was altered by a moderate heat. This points out a further analogy between obsidian and glass, and renders the opinion of those who consider this mineral as of volcanic origin still more probable.

On Thursday, Jan. 11, Dr. Brewster's paper was concluded. He pointed out the analogy between magnetism and heated glass,

and explained several phenomena which had been described in some of his preceding papers. He showed, likewise, that a thermometer might be constructed by means of the different coloured fringes exhibited by plates of glass of various degrees of heat. This thermometer might be made capable of indicating a change of temperature not exceeding one degree of Fahrenheit's thermometer.

At the same meeting a paper by Sir Humphry Davy was read, giving an account of a new method of preventing explosions in coal-mines from fire-damp. His method is to surround the flame of the lamp or candle with a wire sieve, the meshes of which amount at least to 250 in an inch. Such a sieve completely prevents the explosion from setting fire to the gas on the outside of it, even though the most inflammable mixtures of gases, as oxygen and hydrogen, be present. This is certainly one of the most extraordinary and unaccountable facts connected with the propagation of heat and combustion. It is possible (supposing the fact to be correct) that so great an attraction may exist between the wires and the air surrounding them, that the internal combustion and expansion is not able to displace it. If we suppose such a fixedness to exist, it would account for the explosion not kindling the surrounding mixture on the outside of the sieve. This contrivance (supposing it effectual) would completely answer the purposes of the miner. Such sieves might be made for a halfpenny apiece, and they would not in the least obstruct the light, or prevent the candle from being used by the miner as it is at present; whereas the bulk, and little light given out by the lamps, constitutes a serious objection to their use.

LINNÆAN SOCIETY.

On Tuesday, Dec. 5, the remainder of Dr. Acharius's paper describing two new genera of lichens was concluded.

A curious paper was likewise read, giving an account of the ancient inhabitants of Guadaloupe near the spot where the fossil human skeleton was found. Two different tribes existed, to whom the writer of the paper gives the names of Caribes and Galipees. About the year 1710 they quarrelled, and a battle was fought between them on the spot where the skeleton was found. The Galipees were routed, and disappeared in consequence, having no doubt emigrated. The author seems to conceive that the skeletons of the warriors slain in that battle were speedily encrusted with the calcareous sand of the place, and that this recently formed stone constitutes the rock in which the fossil skeleton was found.

On Tuesday, Dec. 19, a paper was read endeavouring to explain the way in which the rock containing the Guadaloupe skeleton was agglutinated. It contained, likewise, an enumeration of the different species of shells and madrepores the fragments of which occur in the rock.

At the same meeting a paper by Dr. Macbride, of South Carolina, was read, giving an account of the fly-catching qualities of the leaves of the *Saracenia flava* and *adunca*. These leaves constitute

a kind of tube with an operculum at the top. They contain a saccharine liquid which allures the insect. It lingers some time on the margin of the leaf, but at last ventures in, and is drowned in the liquid, being unable to make its way up the tube, which is beset with hairs pointing downwards, and preventing its escape. The number of flies destroyed by falling into these leaves is very great. They are sometimes placed in rooms for the purpose of getting rid of flies.

On Tuesday, Jan[illegible] 1816, a paper by M. Richard, of the French Institute, [illegible]ead, containing a description of two new species of American plants, the *xylopia sericea* and *oxandra laurifolia.*

GEOLOGICAL SOCIETY.

June 16, 1815.—A paper, entitled Description of a New Ore of Tellurium, by Professor Esmark, of Christiana (accompanied by a specimen), was read. This ore occurs in hexagonal plates, of a tin-white colour. When exposed to the blow-pipe, it exhibits all the characters of tellurium, and there remains behind a globule of silver. It is found in the Oundal copper-mine, accompanied by copper pyrites and by molybdena.

A paper on the analysis of a Swedish mineral, supposed to be felspar, by John F. W. Herschell, Esq. was read. The former part of the paper consists of observations, supported by examples, for the purpose of showing that silica acts as a weak acid in the composition of mineral substances, and that it combines with the other earths, and with metallic oxides, in definite proportions. The mineral itself, a detailed account of the analysis of which is given in the latter part of the paper, approaches nearly in its composition to fibrolite, its ingredients with their proportions being as follows:

Alumina	64·22
Silica	34·03
Oxide of iron and lime, besides a trace of oxide of manganese and potash	1·75
	100

A letter from S. Solly, Esq. to the Junior Secretary, dated Christiana, Dec. 6, 1814, was read. In this letter some particulars relative to the junctions of the shell lime-stone and trap in the vicinity of Christiana are related, and their application to a particular theory of Mr. S. on the origin of the compact and porphyritic traps.

A paper, entitled An Account of some Attempts to ascertain the Angles of the Primitive Crystals of Quartz, and of the Sulphate of Barytes, by W. Phillips, Esq. M. G. S. was read. M. Haüy, in his Tableau Comparatif, has stated the angles of the primitive crystals of quartz at 94° 24′ and 85° 36′.

Mr. Phillips, in his trials with the reflecting goniometer on some

hundreds of small brilliant crystals from Norway, from Spain, and from Bristol, did not find a single crystal in which the measurement of the angles precisely correspond with those determined by Haüy, nor did he meet with a single example of perfect coincidence among the corresponding angles of any one crystal. The only measurements in which several specimens agreed were 94° 15′ and 85° 45′; and these, therefore, Mr. P. is inclined to consider as approaching nearer to the true dimensions of the primitive rhomboid than any other. This want of coincidence in the measurements of crystals which were selected on account of their brilliancy and seeming perfection, induced Mr. Phillips to subject to similar examination some remarkably fine crystals of sulphate of barytes, and in these also a similar disagreement in the dimensions of the same angle in different crystals was found to occur, amounting to at least 26′. He then examined some good cleavages in the direction of the primitive planes, and found six of them agree perfectly in giving 101° 42′ for the obtuse angle, and 78° 18′ for the acute angle, of the primitive rhomb; a result differing materially from that of Haüy, who states them to be 101° 32′ 13″ and 78° 27′ 47″.

A communication from Dr. Berger, of Geneva, was read. In this paper Dr. B. describes the scapula of some unknown large animal which was recently found in the lake of Geneva.

Nov. 3.—A paper from G. Cumberland, Esq. on certain organic remains found near Weston Super Mare was read. Closely adjacent to Weston Super Mare is a promontory, the summit of which is occupied by a Roman station called Whorlbury Camp; and at the northern extremity of this promontory is a small rocky island, resorted to by fishermen at low water. A narrow horse road leads from the downs above to this island; on the left hand of which, opposite to the sea, may be observed a bed of soft red sand-stone interstratified with others of hard red marl. The entire thickness of these beds is about six feet, they dip at an angle of about 47°, and rest on a grey lime-stone destitute of shells. In the marly part of these beds occur numerous substances, resembling pieces of bamboo separated at the joints. Their length rarely exceeds five inches, but their thickness varies from a quarter of an inch to five inches. Their substance appears to be red clay, more or less penetrated by quartz. There is no apparent passage from one joint to the next, although the ends are often in contact; from which circumstance Mr. C. concludes them to be real vegetable remains. These same substances also occur at Uphill, on the opposite point of Weston Bay, but are there imbedded in a coarse grey shell lime-stone. Just over the outburst of these beds at Whorlbury Camp is a pale yellow sand-stone, containing long white stalks of Abayonia, which when in fragments might easily, from their cellular structure, be mistaken for fossil bones.

In another letter, addressed to the Secretary, Mr. Cumberland mentions the discovery of a new and very elegant bottle encrinite in the black rock of Bristol.

A paper, entitled Some Observations on the Salt Mines of Cardina made during a Tour in Spain in the Summer of 1814, by Dr. Traill, was read. From the bank of the river Cardonero, near the town of Cardona, in the province of Catalonia, a small valley extends for about half a mile in a direction from E. S. E. to W. N. W., bounded by steep and lofty ridges, of a coarse yellowish grey micaceous sand-stone. The bottom and immediate sides of the valley consist of reddish-brown clay, from which large imbedded masses of rock salt project in the manner of more ordinary rocks. At the upper extremity of the valley is a rugged precipice from 400 to 500 feet in height, of greyish-white salt, being perhaps partly natural, but principally artificial, as it forms the side of the great quarry from which this valuable mineral has been extracted during many ages. The lowest part of the present works has a solid floor of pure salt, and is nearly on a level with the bottom of the valley, where no salt occurs, but the real depth of the bed has not been ascertained. The surface of the precipice of salt, which has been long exposed to the weather, is furrowed by innumerable shallow tortuous channels, divided from each other by their edges, often so sharp as to cut the hands like broken glass. This appearance is evidently produced by the winter rains, which in their descent along the face of the rock become nearly saturated with the salt which they dissolve. The general colour of the exposed surface is greyish white, tinged here and there of a pale reddish brown by the intermixture of clay. Towards the extremities of the rock extremely thin layers of plastic clay are insinuated between layers of salt, giving the mass a waved and striped appearance. The fracture of the salt is highly crystalline, and usually exhibits large grained distinct concretions. A brine spring flows out at the foot of the great precipice, the water of which is probably almost saturated, since the channel which it has worn in the salt, over which it has flowed for many years, is not more than two feet wide, and less than a foot in depth. No specimens were observed by Dr. T. of the fibrous variety of salt, nor was there the least appearance of gypsum in the neighbourhood. The salt is quarried by wedges and pickaxes, and when ground in a common mill is perfectly white and fit for use.

Dec. 15.—The reading of Dr. Berger's paper on the Physical Geography of the County of Donegal, in Ireland, which had occupied the Society during the two preceding meetings, was concluded.

The county of Donegal presents an area of about 2,000 square miles, of very varied surface. By far the greater part of this is occupied by primitive rocks, which, rising to considerable elevations above the level of the sea, constitute a very distinct chain of mountains, about 54 miles in extreme length from N. E. to S. W. This chain is itself composed of six nearly parallel and equidistant lines, the entire breadth of which may be stated on an average at about 15 miles. The northernmost parallel, ranging about three miles from the coast, extends from Sheephaven to the Bay of

Giddore. It is composed of several mountainous elevations, more or less connected together; some of which are round backed, do not exceed 800 feet in height, and consist of green-stone; while the others have long flat summits subsiding to the S. W., attain in some parts an elevation of 1,200 feet, are excessively barren, and consist of quartz rock. Of these latter the most remarkable is Caintrena mountain.

The second parallel runs about three miles south of the first, including most of the highest mountains of the county, and is almost wholly composed of quartz rock. It forms a continuous line from Muckish to Arigie, of which the principal summits are Muckish mountain, 2,100 feet high, and exhibiting on its south-western side a wall of quartz rock nearly 1,400 feet high; the three mountains called Aghla, the highest of which rises about 1,900 feet above the sea; and Arigie, the loftiest in the county, being at least 2,400 feet in height.

The third parallel lies at the distance of two or three miles south of the second, and consists entirely of gneiss. It presents round-backed hills more fertile than the quartz rock mountains, and for the most part of inferior elevation. The two mountains, however, of the name of Slieve-Snaght, which belong to this range, must be considered as exceptions with regard to the last particular, as the lower of them is nearly 1,900, and the higher exceeds 2,100, feet in height.

The fourth parallel, almost adjacent to the third, is, like that, composed entirely of gneiss. It is of inferior elevation, the loftiest summit not exceeding 1,700 feet. In parts it is covered by bog; but, upon the whole, offers a large extent of good pasturage.

The fifth parallel is the longest, but the most interrupted, of any. It commences with Binnion Hill, in Innishaven, and extends as far as the entrance of Lough Sivilly. The whole of this range is quartz rock; the summits vary in height from 800 to 1,700 feet, the latter of which is the elevation of Aghla-more, the principal mountain of the line.

The sixth parallel consists of groups loosely connected with each other, some of which (and those the highest) consist of quartz rock. Of these latter Dooghrsey, 2,195 feet above the sea, is the most remarkable.

The spaces which separate the above-mentioned lines of mountain from each other form longitudinal vallies, the course of which nearly corresponds with the bearing or dead level of the strata. From each opening of the valley the ground rises more or less rapidly, but in an unequal proportion, till it attains the summit level in which various springs originate, the waters of which run N. E. and S. W., and are augmented in their progress by the streams discharged into them from the transverse valleys, by which latter the continuity of the main ridges is more or less interrupted. The summit level of the valley between the first and second lines of mountains is 347 feet above the level of the sea; of that between

the second and third lines, 940 feet; the elevation of the others was not ascertained. The mean elevation of the flats and valleys, and champaign land of the county, is about 240 feet.

Most of the individual mountains, and the chains themselves, generally speaking, have a greater declivity to the S. than to the N.; but this declivity is more rapid on the N. than on the S., amounting in the former to eight, and in the latter to only two, feet in 100. The slopes to the N. are encumbered, and rendered uneven, by fallen blocks and bowlders, while those to the S. are quite smooth and even.

A letter from the Rev. Archdeacon Barnes to Mr. Buckland, dated Bombay, March 31, 1815, was read.

In this letter Mr. Barnes communicates, on the authority of Mr. Copland, Assistant Surgeon to the European forces in the Guzerat, some particulars relative to the carnelians of Cambay.

These are all procured from the neighbourhood of Broach, by sinking pits during the dry season in the channels of torrents. The nodules which are thus found lie intermixed with other rolled pebbles, and weigh from a few ounces to two or three pounds. Their colour, when recent, is blackish olive, passing into grey. The preparation which they undergo is, first, exposure to the sun for several weeks, and then calcination. The latter process is performed by packing the stones in earthen pots, and covering them with a layer five or six inches thick of dried goat's dung. Fire is then applied to the mass; and in 12 hours time the pots are sufficiently cool to be removed. The stones which they contain are now examined, and are found to be some of them red, others pink, and others nearly colourless; the difference in their respective tints depending in part on the original quantity of colouring matter, and in part perhaps on the difference in the heat to which they have been exposed.

ROYAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Royal Institute of France during the Year 1814.

Mathematical Part.

By M. le Chevalier Delambre, Perpetual Secretary.

(*Continued from* vol. vi. p. 463.)

M. Mongez, Member of the Class of Ancient History and Literature, has presented to that of the Sciences an antique piece of armour found on the banks of the Somme, and which is formed of a flint fixed in a handle of hart's-horn.

M. Barbie du Bocage, Member of the same Class, has read a memoir of M. le Comte Andreossi on the bosphorus of Thrace, in which there is a long discussion about the actual state of the shore, which ought to furnish important information to judge of the

different systems proposed by philosophers respecting the ancient state of the different seas which at present compose the Mediterranean. This is all that it is possible to say at present of a work merely known to us by a single reading.

M. Rochon, who has supplied astronomers with a more exact method of estimating the diameters of the little planets, has stated to the Class his ideas about extending that method to the diameters of the sun and moon.

M. le Chevalier Delambre has given the description of a sun-dial found at Delos among the ruins of the temple of Apollo, which was brought to Paris by M. Mauduit, jun. architect in the service of the Emperor of Russia, and deposited in the Cabinet of Antiquities. The author of the memoir has taken occasion to treat of the gnomonics of the ancients.

Just when this article was going to press, we were informed that the Antiquities of Athens by Stuart, newly translated into French, Paris, Firmin Didot, 1808, contains a gnomonic monument much more important, more curious, and especially more complete. We have read in the dissertation of Martini, p. 60, that Leroi in his Ruins of the Monuments of Ancient Greece, p. 15, has described a dial which he had seen at Athens, near the house of Thrasillus. Martini adds, that his dial is quite similar to that of Berosus. He says, likewise, that the figure which Leroi has given of it is very incomplete. This prevented us from consulting Leroi, and led us to conclude that Athens offered nothing of this kind worthy of exciting our curiosity. By the advice of M. Visconti, we have consulted the work of Stuart. We there find a very detailed description of a monument known by the name of the Tower of the Winds. It is a regular octagon, on the faces of which are represented the eight principal winds, below which are seen eight different dials, four regular and four declining, at angles of 45°, 135°, 225°, and 315°. The regular dials are the verticals of south, north, east, west; the four others are in the intermediate positions.

Vitruvius, who has described this Tower of the Winds in the sixth chapter of his first book, does not say a word about these eight dials: and, what is singular is, that in the part of his book in which he speaks of all the known dials, he keeps the same silence with respect to the eight dials of Athens, though more important in every respect than those of which he names the inventors. One seems entitled to conclude from this, that the dials have been added afterwards at a time posterior to Vitruvius, and especially posterior to the time of Andronicus Cyrrhestes, author of the monument.

Stuart, who makes himself this objection, endeavours to answer it by a passage of Varro, who, speaking of this tower, denotes it by the name of the Tower of the Clock. This answer, which is far from direct, becomes still less so by the efforts which Stuart makes

to show that the tower contained a water-clock, the vestiges of which still exist in conduits, which he has described with care, and of which he has given figures in two of his plates.

If the tower contained a clepsydra, Varro might call it Tower of the Clock. He would have named it Tower of the Clock, if, besides this clepsydra, it had presented eight other clocks, or solar dial plates.

This curious particularity for the history of gnomonics was a thing sufficiently remarkable for Varro and Vitruvius. We draw little more information from the incomplete expression of the one than from the silence of the other.

The authors of the Historical Dictionary, in speaking of the architect Andronicus, say nothing of the time when he lived. Those of the Universal Biography say that "we judge from the style of architecture of that monument already corrupted, and by the mediocrity of the bas reliefs, that he was after the time of Pericles."

In the time of Pericles and Anaxagoras the science of gnomonics was too little advanced in Greece to enable them to form these eight dials at Athens. Historians speak of the first gnomon established by Anaximander at Lacedemon. There was a great distance between this gnomon, which probably only pointed out mid-day, and the dials declining in various figures, exhibited in the Tower of the Winds. It appears, then, very probable, that Andronicus, or the author of the eight dials, whoever he was, lived a good while after Pericles, who died 429 years before our era. Nothing prevents us from supposing him contemporary with Hipparchus; and then the sun-dials at Athens will suppose nothing that was not known by the works of the ancient mathematicians, of whom Ptolemy has explained and completed the doctrine in his book of Analemma. If there be no contrary proof, I should be inclined to assign as the date the first years of our era. Probably the question will never be resolved. What is certain, or at least very probable, is that these sun-dials suppose a knowledge of gnomonics, and consequently of trigonometry, unless we suppose them to have been traced empyrically by means of the concave hemisphere of Berosus.

These sun-dials are of a form similar to those which we find in the Commentary of Commandin on the Analemma. Their theory is perfectly known. It remained to be known with what precision they had been drawn.

The style is every where wanting. We see only in the marble the holes where it was inserted; but the summit of the style was seldom in the axis of these holes, not even in the regular dials, which are here to the number of four. But the height of the style, and the place of its foot, are not indispensable data; we can deduce them from some of the dimensions of the dial. The author has taken care to mark on his plates the length of a considerable number of these lines; but the choice which he has made is not

always sufficient. It is seldom the most convenient for the calculator. We may even sometimes doubt whether the figures have been transcribed and engraved with the requisite exactness.

Notwithstanding these difficulties, we have satisfied ourselves that the south dial was very exact. The height of the style ought to be $6\frac{1}{2}$ English inches. This value has been directly verified six different ways; we may say by the whole details of the dial. The hours are temporary, and are not numbered.

The northern dial is only a supplement to the first. It is on the same scale, and had the same style. We see only two lines of the morning and two of the evening; or rather those lines which proceed from the bottom of the style indicate the direction of the shadows for these different instants. Two of these four lines are too long, because, instead of the hyperbolic arc, which ought to terminate them, a straight line has been drawn. These slight faults are but of inferior importance.

The east dial plate is not less exact than the south. It is pretty narrow, though the length of the style has been $19\frac{1}{5}$ English inches; that is to say, almost double. This length has been verified by a number of particular proofs, and by the whole of the dial.

The dial of the south-west offers the same agreement in all its parts. The height of its style must have been $25\frac{1}{2}$ inches. The inclination of the equinoctial with the horizontal is 42° 40′, just what calculation gives.

The dial of the north-east does not appear to have been constructed with so much care, or at least so successfully. The style is only $6\frac{1}{2}$ inches. The horary lines, only three in number, are very oblique. The least error in the graphic operation may sensibly alter these lengths, and these considerations excuse the artist. Besides, this dial is the least important of all. We here see nothing which we may not obtain with much more certainty from the neighbouring dials.

The three other dials, those of the south-west, the west, and the north-west, could only offer the counterproofs of the opposite dials. The author has not figured them in his plates. In general, he is frugal of information respecting these dials, which interested him less than what concerned architecture. But he has done all that we could demand in giving us the exact figures of five dials, which offered something particular: these dials do not inform us of any thing which we might not have concluded as well from the dial of Delos; but they are much larger, and better executed. They are in their place, and in all respects form the most curious monument that we know of the practical gnomonics of the ancients.

The following is a list of the works published by the members of the Institute in the course of the year.

Travels to the Equinoctial regions of the new Continent, in the Years 1799, 1800, 1801, 1802, 1803, *and* 1804, *by Alex. de Humboldt and A. Bonpland; drawn up by Al. de Humboldt; with two Atlasses, which exhibit, the one the Views of the Cordilleras*

and the Monuments of the Indigenous Inhabitants of America, and the other the Geographical and Physical Charts. Tom. i. Paris, F. Schvell, 1814.—This first part of the historical relation of a journey, unique in its kind, as it equally interests the naturalist, the philosopher, the historian, the antiquary, the geographer, the astronomer, was expected with so much the more impatience, as it was known to have been ready for some time, and retarded only by circumstances unconnected with the work as well as with the author.

The introduction puts us in mind of the object which M. de Humboldt had in his travels, and collects in a single view the collections and observations which he has made, the care which he took to transport them, the obstacles which he encountered, and the names of the different parts which compose the whole collection.

The first book contains his departure from Spain, his abode at the Canaries, his excursion to the Peak of Teneriffe, the singular observation of sun-rise at a height of more than 1800 fathoms, the duration of which was 8′ 1″, instead of 2′ 41″; the description of the crater, and the magnificent view enjoyed from the summit of the mountain. The traveller discusses the different means employed before him to determine the height of the Peak, gives the history of the eruptions of the volcano, examines its products, which he compares with those of the most celebrated volcanoes which he has visited. He gives an historical account of the Guanches, and examines the remains of their language.

In his voyage from the Canaries to the coast of South America, after some observations on the trade winds, he describes the advantages and inconveniences of different routs which may be taken in crossing the Atlantic. He continually rectifies the estimate of the Pilots by astronomical means, and he announced to them the land from which they thought themselves distant two or three days' navigation. Land appears; the captain takes it for Trinidad, observations pointed out Tobago. The captain is forced to confess his error. After these facts, which would prove, if it were necessary, the importance of the astronomical methods, we find observations, both numerous and interesting, on the temperature of the air and that of the sea; on the colour of the sky and of the ocean; on the inclination of the magnetic needle and the intensity of the magnetic forces; on the purity and electricity of the air.

The second book begins with a description of Cumana and its environs. The frequent earthquakes to which this coast is subject occasions very interesting remarks on these terrible phenomena. The fifth chapter, the last in the volume, is devoted to the salt springs of Araya, to the coast of pearls, and to the ruins of the castle of St. Jago.

The part of the Atlas which is joined to this volume consists of five plates, very well executed, which represent the inferior limit of perpetual snow at different latitudes; the course of the ocean and the province of Varinas; the course of the Rio Meta and the

eastern part of the mountains of New Granada; finally, a geological picture of the singular volcano of Jorullo, which sprung out of the earth in the month of September 1759, surrounded with several thousands of volcanic cones of 100 toises in height.

Tables of the Divisors for all the Numbers of the 2d Million. By M. Burckhardt.—We have already spoken of this work, which appeared on the first of January, 1814. We take the opportunity of announcing that the third and fourth millions are in the press.

Treatise on the Differential and Integral Calculus, by M. Lacroix; 2d Edition, revised and augmented, volume second. About 850 *pages. Paris, Madame Veuve Courcier.*—The author, in a short advertisement, gives the motives which led him to deviate in some points from the plan pointed out in the preliminary discourse to the first volume. In his opinion the general theory of the conditions of integration ought to follow immediately the methods relative to the integration of functions, with a single variable quantity; because it is to this integration that is reduced that of any differential functions whatever, when they satisfy the conditions of integrability. In what concerns partial differential equations, he has made some considerable transpositions, in order to prevent repetition, and to show more clearly the properties and the connexion of the different processes proposed by the great mathematicians of our time, to treat this kind of equations. By this new arrangement, by his remarks on the integrals to which this new edition gives new developements, he has endeavoured to throw light on a subject which had not yet been sufficiently elucidated. He is at pains to point out the difficulty.

The calculus of variations is treated with all the details which the importance and singularity of the method required. To make its nature sufficiently understood, it became necessary to point out the principal attempts that have been made to unite it with the principles of the differential calculus. This the author has executed in his last chapter, where the method of variations is presented in all the generality and simplicity which the first symbols employed by M. Lagrange, and the consideration of infinitely small quantities, give it.

Philosophical Essay on Probabilities, by M. le Compte Laplace. A Volume in 4to. of 96 *pages, Paris, Madame Veuve Courcier.*—M. Laplace, after having in a first work treated this interesting and difficult subject like a consummate mathematician, and having singularly enriched it by new methods, more general and fruitful than those of the great mathematicians who had made it the subject of their meditations, examines it here in a point of view purely philosophical. Without the assistance of analysis, without supposing the reader acquainted with any thing more than arithmetic and the first elements of algebra, he explains the principles and the general results of that theory. He makes it originate from suppositions the most simple, combined so as only to require the degree of attention of which every man is capable, who has the habit of

reflecting. He then applies the principles to the most important questions of life, which, in general, are nothing else than problems of probability. He treats successively of *hope*, of *games*, of the *unknown inequalities which may exist between chances supposed equal;* of *the laws of probability which result from the indefinite multiplication of events*; of *the calculation of probabilities applied to the inquiry into events and their causes*; of *the means which we must choose among the results of a great number of observations*; of *the tables of mortality and the mean length of life*; of *marriages and any kind of associations*; of *benefices depending on the probability of events*; of *the choice and decisions of assemblies;* of *the illusions in the estimate of probabilities*; of *the different means to approach to certainty.*

The work is terminated by an historical notice on the calculation of probabilities, in which the attempts and discoveries of the mathematicians who have applied themselves to this subject are stated and appreciated with great impartiality. Nobody was better entitled than Laplace to draw up this notice, nor more interested that it should be well done.

Theoretical and practical Astronomy, by M. Delambre; *three vols. 4to. Paris, Madame Veuve Courcier.* 1814.—We have announced the abridgement of this work, which appeared in 1813, in one volume 8vo. We have said that the plan of the two treatises is the same. It remains to point out what was suppressed in the abridgement. What distinguishes the complete treatise is, in general, a great variety of questions, of solutions, and formulas, more details on the construction, use, and verification of instruments. Thus, in the article Spherical Trigonometry, will be found a number of formulas, expressing the relation between five and six parts of the same triangle, the analysis of the different methods in use for the resolution of triangles, a more complete and methodical collection of differential expressions, and, finally, a proof triangle calculated with the greatest detail, and which may serve to verify all the formulas imaginable. In the article Gnomonics, besides the new formulas for the description of the horary lines and the arcs of the signs, the whole gnomonic plan is reduced to a single formula, which has only one linear variable quantity, which only affects one of the terms of which it is composed. In the article Refractions, there is a synthetical solution of the problem of the shortest twilight, much more complete, and much easier, than all those drawn from the differential calculus. The chapter on The Diurnal Motion offers solutions, mostly new, of the most useful problems; that of corresponding heights is terminated by tables of correction of a new form, followed by useful remarks on the use of logarithmic tables, which we wish to substitute for tables calculated in natural numbers. When treating of The Elliptical Motion of the Earth, will be found the comparison of the different hypotheses contrived to account for the inequalities of the sun, new solutions of the problem of Kepler, a great number of elliptic formulas, and

all those of M. Gauss, otherwise demonstrated and brought to a notation, which it has been attempted to render more clear. All these formulas are illustrated by numerical examples. The same attention has been paid to the parallaxes, the eclipses, and to every thing that concerns the planets and the comets. In the article Tables of the Sun will be seen the means employed to construct the last tables, the reductions applied to observations made with the repeating circle, the method of observing an equinox and a solstice, with tables to facilitate all these operations. The Theory of Eclipses is explained in different manners, partly new, which have the advantage of being easier and more general. The application of them has been made to the eclipses of 1764, of which it has served to determine the curves of entrance and exit.

In the chapter on The Planets is shown how by means the most simple we obtain the first approximation of an unknown orbit, and how this first sketch may be afterwards perfected. The Transits of Mercury and Venus are treated in a way altogether new, which leads by a more direct and certain way to a knowledge of the parallaxes. In the article Rotation, seven different solutions of that problem are collected, and, for an example, are taken eleven observations of a spot of the sun. The Theory of Saturn's Ring, which terminates this chapter, offers the most exact and simple means, either to predict the phenomena, or to determine the elements from observations. The chapter on The Comets is long. The known formulas are presented so as to serve equally for the parabola and the ellipse. The methods are explained properly to facilitate the construction and ensure the accuracy of the general tables. Examples are given, calculated according to the most accredited methods; and one is explained, which recommends itself by the following advantages: it employs only the calculations most familiar to the astronomer; the longitudes and latitudes observed are, as in all the other methods, the primary data; but they enter only into the first calculations; the succeeding ones employ only heliocentric places; and the elements found may be perfected by the totality of the observations, by equations nearly of the same conditions as those of the planets: the calculations are made as the observations succeed, and the astronomer who has discovered a comet, may on the day itself of the third observation, ascertain its elements. This chapter is terminated by a catalogue of the orbits of all the comets hitherto observed, and by the general tables of the parabolic movements of different form, according as we take for data the anomaly or the days since the perihelion. In the chapter on The Measure of the Earth will be found new calculations on the irregularities of the arc of the meridian, measured in France and in Spain. In the chapter on Nautical Astronomy there are numerous solutions of the most important problems; and the work terminates by a chapter on The Calendar, in which we must point out a fault in the drawing up, which may in some rare circumstances occasion an error of seven days in the result of the calculus. We

allude to the formulas given by M. Gauss to find Easter for any year of the Italian and Gregorian calendar. We satisfy ourselves with noticing here that the formulas are not complete, we shall republish them elsewhere with more exactness, and we shall join to them new formulas still more expeditious, and which, besides, give us the dominical letter, the golden number, and the epact of the year. See the Connoissance Des Temps of 1807, p. 307.

Exercises in the Integral Calculus. Fourth Part, by M. le Chevalier Legendre. This new part is divided into two sections. In the first M. Legendre has completed the theory which he explained in the second part of his exercises. He has particularly attached himself to explain with all the requisite minuteness the properties of a function, which is the mutual connecting link of a multitude of transcendental quantities, and the source from which flow all the formulas which concern the comparison of these transcendentals, their reduction, and their evaluation. The author has already satisfied himself, that he was not mistaken when he hoped that this theory, considered under a new point of view, and augmented by a great number of new formulas, might fix the attention of mathematicians, and that they would see a new branch of analysis brought almost to perfection.

To extend further the applications of this theory, he has inserted at the end of the first section a more extended table than that which terminated the second part. The new logarithms are exact to the 12th decimal, in order that the cipher of the last order may never be in error more than one unity, or at most two. This has given occasion to rectify, and almost to double in extent, a table which Euler had given in his differential calculus, for the sums of the reciprocal powers of the natural numbers.

In the second section will be found different researches, which form a sequel to the third part. A great number of formulas are demonstrated, either entirely new or recently discovered. Among the last are several definite integrals given by M. Bidonc, in the Memoirs of Turin. The author has also presented some new views on the summation of different series, and on the formulas which serve to give the sum of a series of which the general term is given.

It is impossible for us to dwell longer on a work of pure analysis, and almost entirely composed of formulas. See what we have said formerly on the first part of this work. We shall take this opportunity to rectify a passage in our notice of 1810.

In giving an account of the second memoir on Elliptical Transcendentals, we have denoted by the word *Loxodromic*, a species of spiral which M. Legendre there considers, and one of the properties of which is to be the shortest road between two points situated under two different meridians or parallels. This acceptation of the word Loxodromic is not that of navigators and mathematicians; but it appeared to us more conformable to the etymology of the word, which is an *oblique route.* There is no straight

or orthogonal route, but that which takes place in the direction of a meridian or parallel. In the first case we cut all the parallels at right angles, in the second all the meridians. Every other route would traverse both under angles almost always oblique. The spiral considered by M. Legendre had been already analyzed by M. Duscjour, who had given the formula of the variable angle, and remarked further, that this curve would cross the equator in different points at each demi-revolution which it made round the spheroid. It was from this singular property, as well as from the continual variation of the angle, that it appeared to us to merit the name of Loxodromic. The Loxodrome of sailors, on the contrary, makes a constant angle with all the meridians which it crosses, and it is not the shortest road between two given points. Here, then, are two striking differences between the two curves. Hence they ought not to be confounded. But the danger is not great, as the one is only employed in navigation and the other in geodesy; so that the difference of the problems points out sufficiently the choice of the formulas. For the Loxodromic of sailors, see the 36th chapter of our Treatise on Astronomy.

Præcipuarum Stellarum inerrantium Positiones mediæ ineunte Sæculo 19, *ex Observationibus habitis in Specula Panormitana ab Anno* 1792 *ad Annum* 1814, *ex regia Tygographia Militari.*—This new work of M. Piazzi is dedicated to the Institute of France, of which the author is one of the oldest correspondents. In this new edition, in which the number of stars is 7646, without counting those whose positions have not yet received the greatest degree of precision, M. Piazzi has not chosen to adopt any thing which he had not himself verified. He determined the right ascension of the fundamental stars by a direct comparison with the sun. The others have been deduced, as usual, by the difference of their passage over the meridian, observed a great number of times, and the mean result has been taken. Unfortunately, the size of the volume, and unfavourable circumstances, have obliged him to suppress all these comparisons, and even the observations on which they are founded. The declinations suppose the mean refractions of the fifth book of the work on the observatory of Palermo, the latitude 38° 6′ 41″, and a total precession of 58·388″, which leaves 50·2066″ for the precession in longitude.

The annual motions comprehend the proper motions, whenever it was possible to find in the ancient catalogues positions sufficiently certain.

The notes which accompany this catalogue offer many curious remarks on the stars, whose motions had not yet been observed, or of which the brilliancy appears to increase and diminish periodically.

This extract, which we have been obliged to abridge, will be sufficient to show how precious this new catalogue must be to astronomers, who already made constant use of the first edition.

The following memoirs have been approved by the Class :—

Memoirs relative to the Integration of Equations with partial Differences, by M. Ampere. Commissioners, MM. Legendre, Arago, and Poisson, reporter.

The author explains, in the first place, the general considerations which belong to equations of all orders, and which he afterwards particularly applies to those of the first and second order. On this subject, the difficulty and importance of which are known to mathematicians, such considerations are not without utility, and may serve to elucidate some points of theory, even when they do not lead to a new method of integration. This memoir, of which it is impossible here to give the analysis, contains, says the reporter, new and interesting views respecting the calculus of partial differences; and the conclusion of the report is, that the author should be induced to continue these researches, and to connect them to some one of the applications of analysis to mechanical philosophy.

Memoir on the Integration of Equations with partial Differences, by M. Ampere. Commissioners, MM. Arago and Poisson, reporter.

The author considers a class of equations with partial differences of the second order with three variable quantities; namely, linear equations with respect to their greatest differences. The most general of this class contains four terms, three of which are multiplied by second differences, and the fourth is independent of them. The coefficients of these four terms are any functions of the three variable quantities, and of the first two differences. M. Ampere proposes to transform this equation into another, which contains only a single second difference, and he succeeds in his attempt. For that purpose it is necessary, in the case considered by M. Ampere, to change at once the three variable quantities; and the choice of the unknown quantity which must be taken for the principal new variable quantity, constitutes the difficulty of the problem. He gives in his memoir different examples of equations which become entirely linear by means of his transformation, and consequently integral, by the methods known. Thus, concludes the reporter, though this transformation be not always practicable, it gives, however, a real extension to the means of integration known at present. It may be often useful, and contribute to the progress of this part of the science. In consequence, the memoir has been thought worthy of entering into the collection of those which have been approved by the class.

Memoir on Definite Integrals, by M. Cauchy, Engineer des Ponts et Chaussées. Commissioners, MM. Lacroix and Legendre, reporter.

As it is impossible to give an analysis of this work here, we shall satisfy ourselves with stating the conclusions, which are sufficiently extensive to save us a formal extract.

We shall not examine if the new methods of M. Cauchy are more simple than those previously known; if their application is easier, and if they are capable of leading to results which the

known methods could not give; for though we should answer negatively to these questions, the author will still retain the merit:

1. Of having constructed by an uniform method a set of general formulas proper to transform definite integrals, and to facilitate their determination:

2. Of having first remarked that a double integral, taken within given lines for each variable quantity, does not always give the same result in the two ways of effectuating the integration:

3. Of having determined the cause of this difference, and of having given its exact measure by means of *singular integrals*, the idea of which belongs to the author, and which may be regarded as a discovery in analysis:

4. Of having given by his methods very remarkable new formulas of integrals, which may indeed be deduced from known methods, but which nobody had hitherto obtained.

It appears to us, on all these accounts, that M. Cauchy has given in his researches on definite integrals a new proof of the sagacity which he has shown in several other of his productions. We think, then, that his memoir is worthy of the approbation of the Class, and to be printed in the collection of Savans Etrangers.

Memoirs of M. Jacques Binet, *which treat of the Analytical Expression of Elasticity, and of the Stiffness of Curves of Double Curvature.* Commissioners, MM. Carnot and Prony, reporter.

After an introduction purely geometric, containing formulas partly new, relative to polygons whose sides are not all in the same plane, and to curves of double curvature, the author, passing to problems of equilibrium, which constitute the particular object of his paper, successively introduces the consideration of the action of forces upon a polygon of the kind of which we have just spoken, and upon a curve of double curvature, establishing in both systems a theory applicable both to the case of stiffness and to that of elasticity.

The nature of these researches renders it impossible for us to analyse the memoir. We shall be even obliged to abridge the observations of the Commissioners. We shall say only, with them, that M. Binet has the merit of having introduced explicitly and completely into his analysis all the elements of the question of which he treated. We say *explicitly*, to distinguish the method which he has followed from that pointed out by Lagrange's beautiful method of indeterminate quantities. M. Binet combines from the first with the external forces those which he calls *internal*, which represent the effects of the three elasticities of an elastic curve, or the different efforts which tend to change the form of a stiff curve. In this way we know beforehand, and never lose sight of, the signification of the signs which represent each quantity. The function which fills that quantity in the system is always known without equivocation.

However, by this method of proceeding it is necessary to give an account beforehand of all the phenomena to which the combined

play of forces and resistances may give place, which is not always easy. While the method of indeterminate quantities, which requires only the knowledge of the conditions to which the composition of the system is subjected, is what may be called a general instrument, and of a usage always certain, which conducts the analyst by the simple mechanism of the calculus to the discovery of those quantities representative of the different effects supported by the system. But if the method of Lagrange dispenses with regard to these indeterminate quantities with a preliminary exercise of the judgment, on the other hand it requires considerations often delicate in their explanation when we have equations containing them. From this it may happen sometimes that the explanation of which we speak is neglected or omitted, which renders the solution incomplete. The problem of the equilibrium of the curve, whether rigid or elastic, furnishes us with an example of this. Lagrange arrives at three equations absolutely identic with those of M. Binet. These equations contain three indeterminate quantities, which ought to be referred to the extensibility, flexibility, and tortion. But this explanation is not found in the Mecanique Analytique, and it must have in general escaped those who have perused that book. The reporter is even of opinion that Lagrange has not paid attention to the part which these three indeterminate quantities play in his system, considered under a mechanical point of view; for he has not said that the last two would furnish infinite forces, the one of the first, the other of the second, order; a circumstance which must have appeared singular to him if he had not overlooked it. M. Binet deduces these values very simply from his analysis, and solves very clearly the kind of paradox which they present; so as to leave nothing to desire respecting the value, the signification, and the functions of these quantities. If to these considerations we add the remarkable indetermination of the internal forces, which he first, as far as we know, pointed out in treating of the equilibrium of the polygon, we shall conclude from them that he was in the right to announce that his researches might serve as an explanation and supplement of several chapters in the Mecanique Analytique. In general the analysis is managed with much skill; and the geometrical introduction, which would itself be an interesting memoir, ought to confirm, and even increase, the good opinion which has been formed of his scientific merit, from the different works which he has before submitted to the judgment of the Class.

In consequence of this report, the Class, in praising the memoir, ordered it to be printed in the collection of Savans Etrangers.

Memoir relative to the Stability of Floating Bodies, by M. Charles Dupin, Captain of the First Corps of Maritime Engineers, and Chevalier of the Legion of Honour. MM. Sané, Poinsot, and Carnot, reporter.

This subject, treated by Euler and Bouguer, appeared exhausted. M. Dupin, by employing a method not known in the time of these two illustrious mathematicians, has obtained new results. The

manner in which he considers his subject enabled him to find in the first place all the theorems already known, and then many other theorems which are new. We shall quote the following, which is at once simple and remarkable. *According as the position of the equilibrium is stable or not stable, the distance of the centre of gravity of that body from the centre of the heel is either a minimum or a maximum, with respect to all the neighbouring positions which floating bodies can take.* And this. *The directions of the greatest and least stability of any body whatever increase always at right angles.* The conclusion of the report is that the new work of M. Dupin confirms the hopes which this philosopher has already given by his first labours. We cannot but applaud his continual efforts to direct their results towards the practice of the great art to which he has devoted himself.

Small Machine for grinding Corn, for the Use of the Armies, by M. Cagniard Latour.

The conclusions adopted are that the machine is good, that it may be useful in towns or citadels besieged, and to private families in times of scarcity, or when the mills are stopped by ice, and labourers are not employed.

Greek, Latin, and French Edition of the 15 *Books of the Elements, and the Book of the Data of Euclid*, by M. Peyrard. Commissioners MM. Prony and Delambre, reporter.

The Class had already given its approbation to a complete translation of all the works of Euclid remaining to us. M. Peyrard, author of this book, had compared the 23 Greek manuscripts which are in the king's library. The result of this comparison was, that none of these manuscripts is entirely conformable to the Oxford edition; that this edition, which is considered the best, and which is without doubt the most beautiful, is only, as far as the Greek text goes, a copy of the edition of Bale, from which it has taken even the most obvious faults; that most part of the manuscripts offer variations which fill up some blanks or elucidate some passages of the two principal editions; but that in general all these manuscripts differ little from each other, but considerably from the oldest manuscript marked No. 190, and taken from the library of the Vatican, from which it was sent into France by M. Monge. The text in it appears more pure, more clear, less prolix, and more intelligible. M. Peyrard has attached himself chiefly to this manuscript, and has mostly followed it in the edition of the Greek text, which he has joined to his Latin aud French translations.

At the request of his Excellency the Minister of the Interior, the Class has named a commission to examine the fidelity of the translation, and the merit of the numerous variations which M. Peyrard has introduced into the text, or rejected at the end of the work. Another commission of the class of history and ancient literature had been at the same time invited by the Minister to consider the new translation relative to style and execution. The reporters of both Classes, after several conferences, were of the same

opinion respecting the utility of the new edition. The Class of sciences heard and approved a long report in which the edition of M. Peyrard was examined in the greatest detail, and the conclusion of which is, that this edition is evidently superior to every other, and that the author has done every thing in his power to render it worthy of appearing under the auspices of the king to whom it is dedicated.

Memoir of M. Puissant on the Calculation of Differences of Level in the Spheroid. Commissioners MM. Burkhardt and Delambre, reporter.

M. Puissant employed in drawing up a complete treatise of trigonometry for the use of geodesy, has been led to treat of several questions already solved. He obtains all the known results by methods peculiar to himself. In stating the formulas which constitute the basis of the system of measures, he observes in them a slight error, which, however, is confined to quantities so small, that they are usually neglected. But it is always useful to rectify formulas even in their smallest details, and to point out errors which might be adopted with confidence by all those who occupy themselves rather with the practice than the theory.

Memoir on a new Analytical Method of determining the Effects of Aberration in the Position of the Stars, by M. Puissant; the same commissioners.

Here, as in the preceding memoir, the author, by following routs with which he was well acquainted, arrives at all the known formulas, both for the stars, the planets, and the comets. He had at first demonstrated the whole by processes purely analytical; but afterwards making use of some ideas, of which he points out the source and names the author, he has been able to abridge his demonstrations without altering their character.

These two memoirs, approved by the Class, will constitute a part of the treatise on trigonometry promised by M. Puissant.

Determination of the true and apparent angular Distances of Centres of the two Stars submitted to the Influence of Parallaxes, by M. Henry. Commissioners MM. Arrago and Delambre, reporter.

We have seen the analytical processes substituted for the notions of elementary geometry in the demonstration of the most usual formulas of astronomy. On the contrary, we see here spherical trigonometry put in place of pure analysis to demonstrate the formulas by which M. Lagrange solved the problem of the eclipses of the sun, the stars, or planets. In a theoretical point of view the method of Lagrange had obtained the suffrages both of astronomers and mathematicians; but in practice it was soon perceived that these formulas, so beautiful and so general, were very inconvenient, on account of that generality itself. To solve a problem so much above his means, the astronomer divides it into several others, which do not offer the same difficulty. He endeavours to multiply the natural data; by calculating the horary or nonagesimal angle

he determines the parallaxes and the apparent places of two stars, referred to the equator or the ecliptic. It is then only that he endeavours to find the apparent distance, and he obtains it by two lineary formulas of the most simple kind, as well as all those through which he has successively passed. M. Lagrange, on the other hand, attacks the difficulty at once, and without supposing any thing, except what he takes directly from the astronomical tables, he expresses the tangent of the apparent distance of the centres. But this expression is embarrassed by radicles; the quantities under the sign represent values very long and very complicated. The author to no purpose exhausts all the resources of his art and his genius to eradicate from these formulas all the terms, the absence of which will produce scarcely any change in the degree of precision. To no purpose has he contrived tables of an ingenious construction, in order to diminish the length of the calculation; even these tables, and the artifices of the calculus, are tacit acknowledgements that the problem surpasses the force of analysis, and a disguised imitation of the practice of astronomers. These tables, in fact, are formulas of the parallaxes from which the nonagesimal and its height are eliminated, which only makes the use of them more troublesome. It is this task, so great and so difficult, that M. Henri has accomplished by means quite different. M. Lagrange expressed by rectangular co-ordinates the true and apparent positions of two stars and those of the observer in space. M. Henri draws the same expressions from trigonometry, either plane or spherical. By this means he obtains all the formulas of Lagrange; so that the solution has gained nothing on the side of facility. To abbreviate, he restores the nonagesimal eliminated by Lagrange. Without using the name parallax, he introduces what is equivalent, and reduces it to tables; but notwithstanding all these efforts, he acknowledges himself that the method is still very troublesome. He does not believe that any professed astronomer will ever prefer it; but if the method is long and troublesome, this is the only fault with which it can be charged. It is neither less precise nor less proper to give exactly the difference of longitude between two places where the same eclipse shall have been observed. And the new point of view, under which M. Henri has presented it, cannot but augment the number of its partisans, by increasing the number of calculators capable of appreciating it. The methods employed by M. Henri deserve to be generally known. Opportunities perhaps of applying them more advantageously will occur. And the Class, as well as the commissioners, were of opinion, that at at a time when the original memoir of Lagrange is printed in the Connoissance des Temps for 1817, of which only a German translation had appeared in the Ephemerides of Berlin, astronomers would see with pleasure the same formulas demonstrated in a manner quite different, which is neither less rigorous nor less easy.

Annals of Mathematics, pure and mixed; a periodical Work, edited by M. I. D. Gergonne, Professor of transcendental Mathe-

matics in the Lyceum of Nimes, Secretary and Supplemental Professor of Philosophy of the Faculty of Letters of the Academies of Gard, Nancy, and Turin. Nimes, Madame Veuve Belle; & Paris, Madame Veuve Courcier.

The physical and mathematical works presented to the Class are always enumerated in the accounts of the meetings, faithfully and honourably placed in the library, letters of thanks are sent to the authors, and complete lists of all these presents are printed in several of our volumes. But these lists can only contain the titles of the works, the names of the authors, and the time of their reception. There are, however, productions, and especially collections, which would deserve a more particular notice, either on account of their importance, or of the names of their editors. Such in particular are the Annals of Mathematics, the idea of which has been conceived, and the execution followed up, with a perseverance worthy of praise, by two distinguished members of the University of France, MM. Gergonne and Lavernede, powerfully and usefully seconded by several of their worthy colleagues or other professors of celebrity, such as MM. Kramp, Français, brothers Encontre, Du Bourguet and Servois; and likewise by several correspondents of the Institute, among whom we may mention MM. Tedenat, Flaugergues, and Lallemand.

These Annals are chiefly devoted to pure mathematics, and especially to researches having for their object to perfect and simplify the method of teaching the science. Nothing is excluded which may give an opportunity of applying them to the different branches of the exact sciences. Articles occur which will interest the mechanical philosopher in general, and likewise on optics, acoustics, astronomy, geography, fortification, seamanship.

Each number gives one or more theorems to be demonstrated, one or more problems to be resolved. Likewise a list of the new mathematical books, both foreign and domestic.

To the memoirs which they insert and the solutions that are sent them, the editors often add their own reflections and interesting researches. A part containing 32 pages appears each month. Two years form a large quarto volume, and the work is now in its fifth year. Among the great variety of objects discussed in this work, and among which it would be difficult to make a choice, we shall satisfy ourselves with pointing out for the meditations of mathematicians, the memoir of M. Servois on the different systems explaining the principles of the differential calculus, and several memoirs in which M. Kramp gives new analytical solutions of the most important problems of astronomy. What he proposes for the comets or planets newly perceived has this in particular, that it informs us whether the orbit be elliptical, parabolic, or hyperbolic. The author applies these formulas to the comet of 1781, calculated in the parabola by the method of Laplace, and he obtains very different results. These orbits conduct him to a hyperbolic orbit. The distances of the aphelia and perihelia are 1,633934, and

1,048364. This last in the parabola had been found 0·9609951, less of consequence by 0·087367. This is about $\frac{1}{12}$ of the great semiaxis of the earth's orbit. The longitude of the node is 13° 56′ 43″ greater than in the parabola. The inclination is also greater in the hyperbola, but only 36 minutes. It would have been curious to have found at the end of the calculations the comparison of the two orbits with the observations. Mechain had made 12, and Pingré assures us, that the errors of the parabola did not exceed a minute and a half. M. Kramp has only employed three; the total interval is only eight days. By another combination the interval is reduced to five days. Perhaps so small an arc is not sufficient to warrant the conclusion that the orbit is really hyperbolic. But whether parabolic or hyperbolic, we can have no hopes of seeing the comet again, and therefore the question will always remain undecided. Astronomers will learn with interest that M. Kramp announces a sequel to this third memoir.

At the last meeting of the year, on the 26th of December, M. Desmarets read a Memoir on the Tides in the English Channel. From the soundings given in the French Neptune, and the charts of Dr. Halley, the author begins by giving a general plan of the basin. He determines the different depths of the waters of the sea, both towards the two coasts and in the middle of the channel. From these data, from the situation of the coasts of America, and the effects of the luni-solar attraction, combined with the general motion of the waters of the sea, M. Desmarets derives an explanation of the considerable tides observed on the coast of Britanny. We want room for a more exact analysis, and therefore refer to the memoir itself.

M. Biot at the same meeting presented to the Class new researches on the phenomena and laws of polarisation.

M. Burckhardt has communicated new calculations respecting the comet of 1786. This comet could only be twice observed, which, as is well known, is not sufficient for determining its orbit. As a substitute for the third observation, M. Burckhardt makes the most probable suppositions for the distance of the comet from the earth. These different hypotheses conduct him to four orbits, the difference between which are sufficiently small to induce us to hope that the comet might be recognised in case it should return. The author of this memoir, who has much practice in these calculations, regarded generally as very troublesome, and which he is better able to abridge than any other person, endeavours to draw every advantage from this facility. He does not wish to allow any thing to be lost, and endeavours to supply what is wanting to us. In this view he has examined what was the greatest distance which could be supposed between the earth and the comet. He has found that it could not exceed 0,942. In that case indeed the elements would undergo pretty remarkable alterations; but this extreme case is very little probable. His worthy associate M. Buache, entering into his views, and seconding him with equal zeal, is consulting the journals

of navigators at that period. A third observation will perhaps be found, which, though not very precise, will be sufficient at least to reduce the uncertainty within much narrower limits; an uncertainty which is to be apprehended from two observations separated from each other only by an interval of two days.

Article XIV.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

Dr. Merriman's Lectures on Midwifery, at the Middlesex Hospital, will recommence on Thursday, Feb. 8, at half past 10 o'Clock.

Medical School of St. Thomas's and Guy's Hospitals.—The Spring Courses of Lectures at these adjoining Hospitals will commence the beginning of February, viz.:

At St. Thomas's.—Anatomy and the Operations of Surgery; by Mr. Astley Cooper and Mr. Henry Cline.—Principles and Practice of Surgery; by Mr. Astley Cooper and Mr. Henry Cline.

At Guy's.—Practice of Medicine; by Dr. Babington and Dr. Curry.—Chemistry; by Dr. Babington, Dr. Marcet, and Mr. Allen.—Experimental Philosophy; by Mr. Allen.—Theory of Medicine, and Materia Medica; by Dr. Curry and Dr. Cholmeley — Midwifery, and Diseases of Women and Children; by Dr. Haighton.—Physiology, or Laws of the Animal Economy; by Dr. Haighton.—Structure and Diseases of the Teeth; by Mr. Fox.

N. B. These several lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a complete Course of Medical and Chirurgical Instruction.

Russell Institution.—A Course of Lectures on Electrical Philosophy, with its application to the improvement of Chemical Science, and the explanation of Natural Phenomena, will be commenced at this Institution by Mr. Singer, on Monday, Feb. 5, at eight o'clock in the evening.

These Lectures will be continued on the succeeding Mondays at the same hour. They will embrace the most important features of this interesting branch of Natural Philosophy, with occasional observations on the Sciences with which it is most immediately connected.

II. *Coal Gas.*

The Coal Gas Company in London have lately very much increased the gaseous product yielded by coal, by distilling a second time the tar which is obtained during the first distillation. This second product of gas they consider as purer and better than the first product. Many years ago I made various experiments on

the quantity of gas yielded by coals. I found that the tar could be almost completely converted into gas during the first distillation, by making the whole pass through a red-hot tube. I conceive this method might be economically adopted by the Coal Gas Company. They would probably be able by means of it to obtain by the first distillation double the quantity of gas which they procure at present, and thus save a considerable sum, which they must at present waste on a second distillation.

III. *Condensation of Water on Glass.* By Dr. Wells.

(To Dr. Thomson.)

SIR, *London, Jan.* 5, 1816.

I think it very probable that glass may attract moisture from the atmosphere through some special quality, originating, perhaps, in the alkali which forms a part of it, and that this circumstance occasioned the plate of your electrical machine, as mentioned by you in the last number of your Journal, to be wet, while other bodies, though similarly situated, were dry. The quantity of moisture, however, which you found upon the plate, I would denominate small, notwithstanding that it is called by yourself considerable. For you said, if I recollect rightly, when you related this circumstance to me in conversation several weeks ago, that the moisture on the plate was uniformly diffused over it, which appearance I regard as only the commencement of the formation of dew, agreeably to what I have remarked in the eighth page of my essay. I am of opinion, therefore, that although it should be established by further observations, that glass can attract moisture from the atmosphere, in some way unconnected with its greater cold, still the quantity hence arising will always be very trifling, when compared with what it receives in consequence of its lower temperature.

I am, Sir, your most obedient humble servant,

William Charles Wells.

IV. *Royal Society.*

As the writer of the excellent letter which constitutes the first article of the present number does not appear to be sufficiently aware of the nature and constitution of the Royal Society, it may be proper to say a few words on the subject. The Royal Society consists of an association of Gentlemen for the express purpose of promoting the cultivation of the natural sciences. The expense of the association, which is considerable (for Government, so far from supporting the association, as is done in other countries, charges it with taxes which amount to several hundreds a year), is defrayed by the annual contributions of the members. This circumstance prevents the possibility of conferring the title of Fellow upon any person, however celebrated, unless he petition for it. Such a title would be, in fact, imposing on him a tax of 2*l.* 12*s.* a year, which the Royal Society has no right to do. If, therefore, the mathema-

ticians, as the author of the letter affirms, be not Fellows, the fault is not to be imputed to the Society. Every person who wishes to become a member of the Society must express his desire to be so by presenting a petition signed by three members. If any mathematician of respectability does so, I think I can answer for the result; but if the mathematicians of England do not choose to take such a step, it would be very unjust to blame the Royal Society on that account.

V. *On Ensuring the Attention of Watchmen.* By Mr. R. W. Bauhard.

(To Dr. Thomson.)

MY DEAR SIR,

In a late number of the *Annals of Philosophy* you gave us a drawing of a machine or instrument to ensure the attention of watchmen. I hope I shall not be trespassing too much on your time and limits in sending you the enclosed sketch of what I think an improvement on Mr. Beaufoy's plan, as it registers more effectually the time at which the omission did take place (supposing the watchman not to attend regularly), and is also better adapted to common use, as it only requires a common bell pull handle to be put in some part of the watchman's round, which handle he is desired to pull once every hour, or oftener, if thought necessary. The machine itself may be placed in any part of the house, or other building intended to be watched, and communicating with the handle by means of a wire, as a common bell. The enclosed plan consists of the watchwork of a common clock without the hands. Let B B B B (Plate XLIII. Fig. 3) represent the face of the clock, with the drawing A A A for a dial on the front of it, on the periphery of which are 48 small wire pins made to slide easily into the holes, in which they are fitted. On the upper part of the dial is a small mortise, *a*, in which a hammer, E, comes through at such a height that the dial and pins can pass under it. When not in action, the hammer is attached to the bar, *f*, and suspended to the two pivots, *a*, *a*. At one end of the bar, is a lever, *b*, to one end of which is attached the wire, *g g*, and to the other end the spiral spring, C, the use of which is to elevate the hammer, E, so that the pins can just pass under. If we now suppose the clock put in motion, one pin will be brought under the hammer every quarter. If the wire is now pulled, the pin will be driven down level with the surface, which will show that the watchman did his duty; but if he should come either before or after the time, the pin driven either before or after the hour will show the time he pulled the handle. At the expiration of the watch, the dial is to be taken off, by taking out the pin, *h*, and the pins that have been driven into the surface pushed out to their places again. I have sent a very imperfect sketch; but, such as it is, I hope you will see the idea of it, and Remain, dear Sir, yours truly,

R. W. Bauhard.

VI. *Influence of Galvanism; in an Extract of a Letter from Dr. Redman Coxe, Professor of Chemistry, Philadelphia.*

The extensive influence of galvanism throughout nature is so extraordinary, that we cannot wonder if we think we perceive traces of its agency in cases where it has never yet been proved or suspected to exist. I am led to this observation from some reflections that have lately occurred to my mind, as to the principles of destruction in watches and timepieces. When two dissimilar metals are brought together, with any intervening fluid, will not galvanic action ensue? If so, do we not immediately recognise its agency in the cases above mentioned? Are not the pivots of the wheels (iron or steel) revolving in their small caps of brass, lubricated with oil? How, then, can galvanism fail to be excited? It is true the masses in contact are minute, but in a like degree will be the energy of the action produced. Now if I am correct, does it not follow that, however accurate a watch may be adjusted, and however well it may for a time continue to go; sooner or later the oxide produced must obstruct or annihilate the motion of its wheels; and hence (independently of occasional influence from magnetism in any part of the steel work) a watch must necessarily in a few years become of no use as a timekeeper. In the case of a common watch this is perhaps of little moment, excepting so far as regards the expense of the individual; but in chronometers at sea, intended to subserve the purposes of navigation, this must be regarded as of high importance. A great deal is ascribed to bad oil, and to other causes, all of which may have their influence; but let the oil be ever so free from rancidity, if galvanism is active, deterioration must ensue. Can this cause be any how obviated, supposing the principle to be correct? I know of no mode but that of making the pivots themselves of the same material as that they play in; and as they are so small, it seems to me this may easily be effected by tempering them properly. At any rate, I conceive the circumstance to be of sufficient importance to awaken the attention of those more immediately interested in the construction of this useful machine.

VII. *On the usual Mode of Fixing the common Hour-glass at Sea.* By the Same.

Another point which occurs to my mind, and which I do not recollect to have seen in any author, is the probable influence which, on a great scale, may arise to the perfect notation of time at sea, by the usual mode of fixing the common hour-glass. Is it not probable that during heavy gales the violent shocks sustained by the ship from the waves may momentarily occasion a sudden check to the regular passage of the sand through the small opening of the glass? However trifling in itself, the effect repeated frequently would certainly tend to prolong the apparent period of 24 hours, and thus cause an error in the ship's way. And when neither sun, moon, nor stars, enable an observation to be made, by which such

error may be rectified, can we doubt that evil may be produced thereby? All this may probably be rectified by simply suspending the glass as a compass, so as uniformly to maintain its perpendicular position. This, like the former, is a case to be determined by practical observation, although theoretically we may deem it true.

VIII. *Improvement in the Plate Electrical Machine.* By the Same.

In the plate electrical machine, every one has probably at times found the inconvenience of the usual form of the arms of the prime conductor, which pass on each side of the glass to collect the fluid. There is moreover, without proper care, some danger perhaps of breaking the glass in removing the conductor; and in a variety of ways it is troublesome. I made some short time ago what I consider as an improvement in this part of the apparatus, especially as it can be adapted to every sized plate. It consists simply of a glass rod standing firmly in a loaded foot, on the top of which a cap is fitted of brass, ending in a ball, perforated laterally through the middle. Through this opening a brass rod passes, and is bent on each side, forming two parallel arms about two inches apart. This rod (the arms of it) moves up and down in the opening, but may be fixed at any angle by a small screw at top of the ball. The inner side of the arms are provided with points about half an inch long. Now one of such is placed near each edge of the glass plate, and the arms being moved to the requisite situation, the small screw fixes them by pressing upon the rod. From one of the arms, or from the cap or ball at top, a chain passes to the prime conductor, which may thus be suspended from the ceiling by silk, or fixed in any convenient mode. It is evident that, from the motion of the arms, it may, if made of a good size, be fitted to use as well in a small as a large machine; and when not used, the arms may be allowed to drop laterally, and occupy a very small space in the case for the machine. The following rough sketch will explain the above to your comprehension.

A (Plate XLIII. Fig. 4) represents the machine with the arms shut laterally as placed in case; B, the machine as placed in its relative position on one side of the glass plate, *a*; *b*, a chain to communicate with the prime conductor, placed in any convenient situation.

Having tried the above, I can really testify to its utility, and think it is a very considerable improvement and simplification of this part of the electric machine. It can be set up or removed in an instant; and, being unconnected with the conductor except by the chain, this last may be removed to any distance or situation, so as to guard against accident or danger.

IX. *Use of Galvanism as a Telegraph.* By the Same.

I observe in one of the volumes of your *Annals of Philosophy* a proposition to employ galvanism as a solvent for the urinary calculus, but which has been very properly, I think, opposed by Mr.

Armiger. I merely notice this, as it gives me the opportunity of saying that a similar idea was maintained in a thesis three years ago by a Graduate of the University of Pennsylvania.

I have, however, contemplated this important agent as a probable means of establishing telegraphic communication with as much rapidity, and perhaps less expense, than any hitherto employed. I do not know how far experiment has determined galvanic action to be communicated by means of wires; but there is no reason to suppose it confined as to limits, certainly not as to time. Now by means of apparatus fixed at certain distances, as telegraphic stations; by tubes for the decomposition of water and of metallic salts, &c. regularly ranged; such a key might be adopted as would be requisite to communicate words, sentences, or figures, from one station to another, and so on to the end of the line. I will take another opportunity to enlarge upon this, as I think it might serve many useful purposes; but, like all others, it requires time to mature. As it takes up little room, and may be fixed in private, it might in many cases, of besieged towns, &c. convey useful intelligence, with scarcely a chance of detection by the enemy. However fanciful in speculation, I have no doubt that sooner or later it will be rendered useful in practice.

I have thus, my dear Sir, ventured to encroach upon your time with some crude ideas, that may serve perhaps to elicit some useful experiments in the hands of others. When we consider what wonderful results have arisen from the first trifling experiments of the junction of a small piece of silver and zinc, in so short a period, what may not be expected from the further extension of galvanic electricity! I have no doubt of its being the chiefest agent in the hands of nature, in the mighty changes that occur around us. If metals are compound bodies, which I doubt not, will not this active principle combine those constituents in numerous places, so as to explain their metallic formation: and if such constituents are in themselves aeriform, may not galvanism reasonably tend to explain the existence of metals in situations to which their specific gravities certainly do not entitle us to look for them.

X. *Royal Institute of France.*

Class of Mathematical and Physical Sciences.—Prizes proposed at the meeting of the 8th of January, 1816.

Theorem of Fermat.—Though the successive labours of different mathematicians have advanced the science of numbers far beyond what it was in the time of Fermat, yet two of the principal theorems of that philosopher remained still without demonstration, or at least were demonstrated only in the two first of the general cases which they embrace.

One of these theorems, that which concerns polygonal numbers, has been just demonstrated by M. Couchy, in a memoir which obtained the approbation of the Class, and which will be likewise approved of by all mathematicians.

Nothing now remains to be demonstrated but the other theorem, namely, that *no power exists beyond the square, which can be divided into two other powers of the same degree.*

A demonstration of this theorem for the fourth power was given by Fermat himself, in one of his marginal notes on Diophantus. Euler afterwards demonstrated in a similar manner the theorem as applied to the third power; but we still want a demonstration for the higher powers, or for those whose exponent is a prime number, for from them all the others may be immediately deduced.

In this state of things the Class, wishing to pay respect to the memory of one of the philosophers who have done the most honour to France, and desiring at the same time to give mathematicians an opportunity of completing this part of the science, proposes for the prize of mathematics to be given in January 1818, the general demonstration of the problem stated above.

The prize will be a gold medal of the value of 3000 francs (125*l.*)

Disturbances of the Planets.—The Class of Sciences had proposed as the subject of a double prize, which might be kept in reserve, if necessary, till the first of January, 1816, *the theory of the planets whose excentricity and inclination are too considerable to put it in our power to calculate their disturbances exactly by methods already known.* The Class did not require any numerical application, it required only *analytical formulas, but disposed in such a manner, that an intelligent calculator might be able to apply them with certainty either to the planet Pallas, or to any other hitherto discovered, or which should be afterwards discovered.* The period of five years, which it had assigned, has just expired. Two memoirs only have been received, the authors of which have not conformed sufficiently to the intentions expressed in the annunciation of the prize. Both (especially one) have left too many analytical developements to be executed by the mathematicians who should wish to put themselves in a situation to understand and judge of the solution of the problem which they have given. They have neglected too much to come down to the level of the calculator, who should wish to form tables of Pallas, or of any other planet. The supplements sent at different times are very far from removing all the difficulties.

The Class peceiving by these supplements, and by the notes transmitted by the anonymous authors, that they had not time to enter into all the developements necessary; and considering further, that the same cause might prevent other mathematicians, possessed of the requisite knowledge and abilities to treat so difficult a subject, from coming forward as candidates, has thought proper to prolong the time for deciding the prize for another year. The prize will be voted in the meeting of January 1817, to the paper which shall fully satisfy the conditions above stated. The prize shall be double, that is to say, a gold medal of the value of 6000 francs (250*l.*). The Essay sent to the Institute must be written in

French or Latin, and will not be received after the 1st of October, 1816.

Prize of Galvanism.—Nothing has come to the Institute deserving the annual prize, founded to recompense the labours undertaken in order to advance this important part of science.

Yet the subject is far from exhausted. The Class conceives that it may be necessary to call the attention of philosophers to some of the points still wanting to complete that theory.

The experiments on the action of the two poles of the Voltaic battery, and on its influence in the combination and decomposition of bodies, have been carried very far; but another object, which this naturally recalls, has perhaps been too much forgot. It has been long since observed, that in chemical combinations made without the direct assistance of electricity, and between substances which before their combination give no very sensible sign of electricity, the compounds obtained were in a very evident electrical state, susceptible of accumulation, and of being measured by a condenser.

It would therefore be important, as it has been determined in a great many cases what combinations result from the action of a calculable electricity, to determine on the contrary what measure of electricity results from different combinations in which bodies passed to a sensible and calculable electric state. A tolerably complete set of experiments undertaken with this view would probably possess considerable interest and utility.

Another phenomenon, not less interesting, and which particularly concerns the animal economy, is that which manifests itself when alternate portions of nerves and muscles of the same animal, or of different animals, are capable of forming a circuit, the contacts of which produce the same excitations which result from a circle composed of metals intermediate between the muscles and nerves.

This experiment, which originated with Galvani, and was afterwards repeated by different philosophers, to which obviously belong the phenomena of the torpedo, analysed at last, and reduced under the theory of the electric plates of the pile, by Volta, may perhaps by its developements, and peculiar kind of experiments, be extended and applied to different circumstances of the animal economy, so as to throw new light on the still so obscure theory of the nervous influence on the organic actions, and on the result of these actions.

We shall not attempt to extend this idea further; but it seemed proper to call the attention of philosophers and physiologists to a first experiment, the developements of which have been hitherto neglected, and which is still confined to a first fact, the consequences of which seem capable of being much further extended. The first experiments of M. de Humboldt on the changes which the animal liquors, just after issuing from their vessels, experience from the action of galvanic excitements, are a proof of what physiologists may hope from a new set of experiments undertaken with the views just alluded to.

The Class invites philosophers to undertake these two sets of experiments, which may give a new and important extension to the theory of electricity.

General Conditions.—Every person, the Members of the Institute excepted, may become a candidate. No treatise sent with a view to a prize ought to contain the name of the author, but only a paragraph or device. The author may, if he chooses, send a separate and sealed letter, containing, besides the paragraph and device, the name and address of the author, which will not be opened unless the piece shall gain the prize.

The papers intended for the prizes may be sent to the Secretary of the Institute, paying the carriage of the parcels containing them. The clerks in the Secretary's office will give receipts. They may be sent likewise (carriage paid) to the Perpetual Secretary of the Class.

The candidates are informed that the Institute will not return any of the works sent for the prizes; but the authors shall be at liberty to take copies, if they have occasion for them.

The Administrative Commission of the Institute will deliver the medal to the person who presents the receipt; and in case there be no receipt, the medal will be given only to the author himself, or to his order.

Of these different conditions, the first and the last only apply to the prize of galvanism, and to the medal of Lalande, which may be given to printed works.

Article XV.

Scientific Books in hand, or in the Press.

Mr. T. Heming, of Magdalene Hall, Oxford, has announced for publication a Map of Scriptural and Classical Geography, accompanied by an Historical and Descriptive Volume, in demy 8vo., wherein the Origin of Nations is particularly examined and discussed; intended to facilitate a knowledge of the progressive Colonization of the Earth, and to establish more clearly the Foundation of Universal and Chorographical History.

Mr. Wm. Phillips has now in the Press, and will publish in the course of the ensuing month, an Elementary Introduction to the Knowledge of Mineralogy and of Minerals, including some Account of the Places at which, and of the Circumstances under which, Minerals are found: and Explanations of the Terms commonly used in Mineralogical Description. It will be comprised in a small volume in duodecimo, and is designed for the Use of the Student.

Dr. Granville has in the Press a Translation of that part of Orfila's General Toxicology which more particularly relates to Poisons derived from the Vegetable and Animal Kingdoms. The subject having formed a very important part of Dr. Granville's scientific pursuits, he has been enabled to accompany his translation with copious Notes and Additions.

Article XVI.

METEOROLOGICAL TABLE.

1815.	Wind.	Barometer. Max.	Min.	Med.	Thermometer. Max.	Min.	Med.	Hygr. at 9 a. m.	Rain.	
12th Mo.										
Dec. 23	S W	29·70	29·27	29·485		32		80		☾
24	S W	29 50	29 25	29·375	42	27	34·5	76		
25	S W	29·78	29·53	29·665	36	21	28·5	83		
26	S W	29·53	29·00	29·265	43	40	41·5	80	—	
27	Var.	29·86	29·00	29·430	41	25	33·0	77	—	
28	S W	29·88	29·75	29·815	46	32	39·0	77	·39	
29	N W	30 09	29·88	29·985	50	41	45·5	55	—	
30	S W	30·52	30·09	30·305	43	22	32 5	90		●
31	S W	30·52	30·33	30·425	35	24	29·5	94		
1816.										
1st Mo.										
Jan. 1	S W	30·33	30·20	30·265	39	22	30·5	80		
2	N W	30·20	30·01	30·105	33	21	27·0	90		
3	S W	30·32	30·00	30·160	38	24	31·0	95		
4	S W	30·32	30·14	30·230	37	26	31·5	77		
5	S W	30·14	29·79	29·965	45	34	39·5	91		
6	W	29·78	29·60	29·690	49	35	42·0	61	6	
7	N W	29·78	29·63	29·705	40	32	36·0	94	·13	☽
8	S	29·40	29·30	29·350	49	41	45·0	67	·13	
9	S W	29·34	29·31	29·325	48	41	44·5	70	·13	
10	W	29·31	28·90	29·105	50	41	45·5	61	·24	
11	N W	29·42	28·90	29·160	47	36	41·5	55		
12	S W	29·42	28·96	29·190	43	32	37·5	79	·17	
13	S E	29·15	28·87	29·010	39	29	34·0	86	2	
14	S	29·32	29·26	29·290	43	31	37·0	91	5	○
15	W	29·56	29·08	29·320	42	33	37·5	67	·27	
16	S W	29·35	29·20	29·275	47	34	40·5	75	·44	
17	S W	29·63	29·35	29·490	39	30	34·5	80		
18	S W	29·64	29·60	29·620	42	29	35·5	98	—	
19	S W	29·64	29·45	29·545	40	29	34·5	85	·10	
20	W	29·45	29·16	29·305	38	29	33·5	92	5	
		30·52	28·87	29·615	50	21	36·52	79·5	2·18	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Twelfth Month.—23. A thaw, p. m. with a little rain: windy night. 24. Dew and *Cirrostratus*, a. m.: cloudy at intervals: windy at S.W., yet it froze in the evening. 26. Maximum of temp. at nine, a. m., and beginning to rain: much wind, especially about sun-set. 27. It is said to have lightened much, early this morning: a stormy day, with rain and snow. 28. Temp. at the minimum at nine: snow: sleet: rain. 29. Temp. at maximum at nine: a little rain: a gale through the night. 30. Fine morning, though with a pale sky: *Cirrostratus*, coloured at sun-set: the river (Lea) rose higher, apparently by the tide, than at any time since 1809. 31. Hoar frost: a frozen mist, with *Cirrostratus* above, followed by a fine day.

1816. *First Month.*—1. Misty air: *Cirrostratus*. 2. Hoar frost: a frozen mist, depositing much rime: the middle of the day fine. 3. Fine morning: the roads icy, it having thawed some part of the night. 4. Hoar frost: *Cirrostratus* in flocks: a breeze. 5. Coloured sun-rise: fine, with *Cirrostratus*. 6. Max. temp. at nine: cloudy: the wind rising: very heavy *Cumulostrati*, after some rain: clear windy night. 7. Min. of temp. at nine: elevated *Cirrostratus* in bars, just visible: wind and clouds: a lunar halo. 8. Max. temp. at nine: wet morning. 10. Much cloud, with *Nimbi* forming, p. m.: stormy night. 11. A gale through the day and night: much evaporation evident in consequence: lunar halo. 12. *Cirrostratus* descending from above: a gale, with rain after. 13. Fair day. 14. After frost in the night, a shower early: drizzling, p. m. 15. Overcast with *Cirrostratus*: rain: clear at night. 16. A slight ground frost: large *Cumuli*, mixed with other modifications, p. m. which going off, showed elevated in the N. and N. E.: to these succeeded linear *Cirri*, filling the sky, and crossing each other almost at right angles: these appearances were followed by a most violent storm of wind and rain in the night. 17. Fair: wind at night. 18. Min. temp. at nine: hoar frost: misty horizon: *Cirrocumulus*, followed by denser clouds, and rain at evening. 19. Max. temp. at nine: very misty: at noon a bank of dense clouds of various modifications in the S.: windy at evening: rain in the night. 20. Fair day.

RESULTS.

Prevailing Wind S. W.

Barometer:	Greatest height	30·52 inches;
	Least	28·87 inches;
	Mean of the period	29·615 inches.
Thermometer:	Greatest height	50°
	Least	21°
	Mean of the period	36·52°

Mean of the hygrometer at nine, a. m. 79·5°. Rain, 2·18 inches.

So decided a westerly current has prevailed during this period, that on one night only an easterly wind was rather inferred from circumstances than observed. From the facility with which such a current veers to N. and S. it often makes a very unsteady barometer, as has happened in this case, the changes in its direction having in the whole been nearly equal in number to the days of the period. In the fore part, a double depression was succeeded by a very considerable double elevation, the *maximum* concurring with a remarkably high spring tide. In the latter part the quicksilver took a range below the mean, and went rapidly through a series of sharp depressions, to the number of seven or eight. (The terms elevation and depression are here applied to the whole time taken by the curve in receding from, and returning to, the mean elevation.) The weather has been such as to agree very well with the indications of the instruments: a few days fair, while the barometer was high: the remainder an alternation of gales of wind, rain, and frost: the diurnal temperature at times more under the dominion of the winds than of the sun.

TOTTENHAM, *First Month*, 22, 1816. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

MARCH, 1816.

Article I.

Biographical Account of the late John Robison, LL. D. F. R. S. E. and Professor of Natural Philosophy in the University of Edinburgh. By John Playfair, F.R.S. L. & E. &c.*

THE distinguished person who is the subject of this memoir was born at Boghall, in the parish of Baldernock, near Glasgow, in the year 1739. His father, John Robison, had been early engaged in commerce in Glasgow, where, with a character of great probity and worth, he had acquired considerable wealth, and, before the birth of his son, had retired to the country, and lived at his estate of Boghall.

His son was educated at the grammar school of Glasgow. We have no accounts of his earliest acquirements, but must suppose them to have been sufficiently rapid, as he entered a student of Humanity in the University of Glasgow in November, 1750, and in April, 1756, took his degree in Arts.

Several Professors of great celebrity adorned that University about this period. Dr. Simson was one of the first geometers of the age; and Mr. Adam Smith had just begun to explain in his lectures those principles which have since been delivered with such effect in the Theory of Moral Sentiments, and in the Wealth of Nations. Dr. Moore was a great master of the Greek language, and added to extensive learning a knowledge of the ancient geometry much beyond the acquirement of an ordinary scholar.

Under such instructors, a young man of far inferior talents to those which Mr. Robison possessed could not fail to make great

* From the Transactions of the Royal Society of Edinburgh, vol. vii. part ii.

advancement. He used, nevertheless, to speak lightly of his early proficiency, and to accuse himself of want of application; but from what I have learnt, his abilities and attainments were highly respected by his cotemporaries, and he was remarked at a very early period for the ingenuity of his reasonings, as well as the boldness of his opinions. According to his own account, his taste for the accurate sciences was not much excited by the pure mathematics, and he only began to attend to them after he discovered their use in natural philosophy.

In the year following that in which he took his degree, Dr. Dick, who was joint Professor of Natural Philosophy with his father, died, and Mr. Robison offered himself to the old Gentleman as a temporary assistant. He was recommended, as I have been told, by Mr. Smith, but was nevertheless judged too young by Mr. Dick, as he was not yet nineteen. The object to which his father, a man of exemplary piety, wished to direct his future prospects, was the church; to which, however, he was at this time greatly averse, from motives which do not appear, but certainly not from any dislike to the objects or duties of the clerical profession. It was very natural for him to wish for some active scene, where his turn for physical, and particularly mechanical science, might be exercised; and the influence of those indefinite and untried objects, which act so powerfully on the imagination of youth, directed his attention toward London. Professor Dick and Dr. Simson joined in recommending him to Dr. Blair, Prebendary of Westminster, who was then in search of a person to go to sea with Edward, Duke of York, and to assist his Royal Highness in the study of mathematics and navigation. When Mr. Robison reached London, in 1758, he learnt that the proposed voyage was by no means fixed; and after passing some time in expectation and anxiety, he found that the arrangement was entirely abandoned. This first disappointment in a favourite object could not fail to be severely felt, and had almost made him resolve on returning to Scotland.

He had been introduced, however, to Admiral Knowles, whose son was to have accompanied the Duke of York; and the Admiral was too conversant with nautical science not to discover in him a genius strongly directed to the same objects. Though the scheme of the Prince's nautical education was abandoned, the Admiral's views with respect to his son remained unaltered, and he engaged Mr. Robison to go to sea with him, and to take charge of his instruction. From this point it is that we are to date his nautical, as well as scientific, attainments.

About the middle of February, 1759, a fleet sailed from Spithead under the command of Admiral Saunders, intended to co-operate with a military force which was to be employed during the ensuing summer in the reduction of Quebec. Young Knowles, whom Mr. Robison had agreed to accompany, was a midshipman on board the Admiral's ship, the Neptune, of 90 guns; but in the course of the voyage being promoted to the rank of Lieutenant in the Roya

William, of 80 guns, Mr. Robison went with him on board that ship, and was there rated as a midshipman.

The fleet arrived on the coast of America in April; but it was not till the beginning of May that the entire dissolution of the ice permitted it to ascend the River St. Lawrence, and that the active scene of naval and military operations commenced, which terminated so much to the credit of the British arms. A person whose seafaring life was to be limited to two years may well be considered as fortunate in witnessing during that short period a series of events so remarkable as those which preceded and followed the taking of Quebec. Though great armies were not engaged, much valour and conduct were displayed; the leaders on both sides were men of spirit and talents; and on the part of the English the most cordial co-operation of the sea and land forces was worthy of men animated by the spirit of patriotism, or the love of glory. The fate also of the gallant leader, who fell in the moment of victory, and in the prime of life, by repressing the exultation of success, gave a deeper interest to the whole transaction.

Of the operations of this period Mr. Robison was by no means a mere spectator. A hundred seamen, under the command of Lieut. Knowles, were drafted from the Royal William into the Stirling Castle, the Admiral's ship. Mr. Robison was of this party, and had an opportunity of seeing a great deal of active service. At this time, also, he was occasionally employed in making surveys of the river and the adjacent grounds; a duty for which he was eminently qualified, both by his skill as a mathematician, and his execution as a draughtsman.

It is, however, much to be regretted that his papers, whether memorandums or letters, give no account of the incidents of this period; so that we are left to conclude, from the history of the times, what were the events in which he must have taken part, or to gather, from the imperfect recollection of his conversation, the scenes in which he was actually engaged. I have heard him express great admiration at the cool intrepidity which he witnessed when the fire-ships, sent down the stream against the English navy at anchor in the river, seemed to present a wall of fire, extending from one bank to another, from which nothing that floated on the water could possibly escape. Without the smallest alarm or confusion, the British sailors assailed this flaming battlement in their boats, grappled the ships which composed it, and towed them to the shore, where they burnt down quietly to the water's edge.

An anecdote which he also used to tell deserves well to be remembered. He happened to be on duty in the boat in which Gen. Wolfe went to visit some of his posts, the night before the battle, which was expected to be decisive of the fate of the campaign. The evening was fine, and the scene, considering the work they were engaged in, and the morning to which they were looking forward, sufficiently impressive. As they rowed along, the General,

with much feeling, repeated nearly the whole of Gray's Elegy (which had appeared not long before, and was yet but little known) to an officer who sat with him in the stern of the boat; adding, as he concluded, that "he would prefer being the author of that poem to the glory of beating the French to-morrow."

To-morrow came, and the life of this illustrious soldier was terminated, amid the tears of his friends and the shouts of his victorious army. Quebec fell of course; and soon afterwards the fleet under Admiral Saunders sailed for England. When they arrived on the coast they were informed that the Brest fleet was at sea, and that Sir Edward Hawke was in search of it. Without waiting for orders, Admiral Saunders sailed to reinforce Hawke, but came too late, the celebrated victory over Conflans, in Quiberon Bay, having been obtained (on the 20th of November) a few days before he joined. Whether the Royal William accompanied the rest of the fleet on this occasion, I have not been able to learn. The body of General Wolfe was brought home in that ship, and was landed at Spithead on the 18th of November. From that date to the beginning of next year I find nothing concerning the Royal William, when that ship, with the Namur, and some others, under the command of Admiral Boscawen, sailed on an expedition to the Bay of Quiberon. On this service the Royal William remained between five and six months, having been twice sent to cruise off Cape Finisterre, for five weeks each time.

About this period a series of letters from Mr. Robison to his father begins; and though the letters do not enter much into particulars, they leave us less at a loss about the remaining part of his seafaring life.

On the 3d of August the Royal William returned to Plymouth, the greater part of the crew being totally disabled by the sea-scurvy, from which Mr. Robison himself had suffered very severely. He writes to his father that, out of 750 able seamen, 286 were confined to their hammocks in the most deplorable state of sickness and debility, while 140 of the rest were unable to do more than walk on deck. This circumstance strongly marks to us, who have lately witnessed the exertions of British sailors, in the blockade of Brest, and other ports of the enemy, the improvement made in the art of preserving the health of seamen within the last 50 years. The Royal William, notwithstanding the state of extreme distress to which her crew was reduced, by a continuance at sea of hardly six months, was under the command of Capt. Hugh Pigott, one of the most skilful officers of the British navy. Mr. Robison, indeed, never at any time mentioned his name without praise, for his knowledge of seamanship, and the address with which he used to work the ship in such bad weather as rendered her almost unmanageable to the other officers. The art of preserving the health of the seaman is a branch of nautical science which had at that time been little cultivated. Our great circumnavigator had not yet shown

that a ship's crew may sail round the globe with less mortality than was to be expected in the same number of men living for an equal period in the most healthful village of their native country.

Mr. Robison's letters to his father about this time are strongly expressive of his dislike to the sea, and of his resolution to return to Glasgow, and to resume his studies, particularly that of theology, with a view of entering into the church. These resolutions, however, were for the present suspended, by a very kind invitation from Admiral Knowles to come and live with him in the country, and to assist him in his experiments: "Thus (says the Admiral) we shall be useful to one another." What these experiments were is not mentioned, but they probably related to ship-building, a subject which the Admiral had studied with great attention. Mr. Robison accordingly continued to enjoy a situation and an employment that must both have been extremely agreeable to him, till the month of February in the year following, when Lieut. Knowles was appointed to the command of the Peregrine sloop of war, of 20 guns. Whether the plan of nautical instruction which Mr. Robison proposed for his pupil was not yet completed, or whether he had after all come to a resolution of pursuing a seafaring life (of which there is an appearance in some of his letters), he embarked in the Peregrine, and he even mentions his hopes of being made purser to that ship. The first service in which Capt. Knowles was employed was to convoy the fleet to Lisbon. In a letter from Plymouth, where they were forced in by the weather, Mr. Robison paints in strong colours the difference between sailing in a small ship, like the Peregrine, and a first rate, like the Royal William, and the uncomfortable situation of all on board, during a gale which they had experienced in coming down the Channel. The voyage, however, gave him an opportunity of visiting Lisbon, on which the traces of the earthquake were yet deeply imprinted; and the ship continuing to cruise off the coast of Spain and Portugal, he had occasion to land at Oporto, and other places on the Portuguese coast. In the month of June he returned to England; and from this time quitted the navy, though he did not give up hopes of preferment. He returned to live with Admiral Knowles, and in the end of the same summer was recommended by him to Lord Anson, the First Lord of the Admiralty, as a proper person to take charge of Harrison's time-keeper, which, at the desire of the Board of Longitude, was to be sent on a trial voyage to the West Indies.

The ingenious artist just named had begun the construction of his chronometer on new principles as early as the year 1726, and, with the fortitude and patience characteristic of genius, had for 35 years struggled against the physical difficulties of his undertaking, and the still more discouraging obstacles which the prejudice, the envy, or the indifference of his cotemporaries, seldom fail to plant in the way of an inventor. Notwithstanding all these, he had advanced constantly from one degree of perfection to another, and it was his fourth time-keeper, reduced to a portable size, and im-

proved in all other respects, that was now submitted to examination. It was intended that Mr. Robison should accompany young Harrison and the time-keeper in a frigate, the Deptford, to Port Royal, in Jamaica, in order to determine, on their landing, the difference of time, as given by the watch, and as found by astronomical observation. The time-keeper accordingly was put into the hands of Mr. Robertson, of the Naval School at Portsmouth, who determined its rate, from nine days that it remained in his custody, to be $2\frac{2}{3}''$ slow per day, and also the error to be $3''$ slow on the 6th of November at noon, according to mean solar time.

The Deptford sailed on Nov. 18, and arrived at Port Royal on Jan. 19; on the 26th Mr. Robison observed the time of noon, and found it to answer to $4^h\ 59'\ 7\frac{1}{2}''$ by the watch; and this being corrected for the error of $3''$, and also for the daily accumulation of $2\frac{2}{3}''$ for $81^d\ 5^h$, the interval between the observations, the difference of longitude between Portsmouth and Port Royal came out $5^h\ 2'\ 47''$; only $4''$ less than it was known to be from other observations.

The instructions of the Board further required that, as soon as an opportunity could be found, the same two Gentlemen should return with the watch to Portsmouth, that, by a comparison of it with the time there, the total error, during both voyages, might be ascertained. The opportunity of return occurred sooner than they had any reason to expect; for the Spanish war having now broke out, an alarm of an invasion of Jamaica from St. Domingo occasioned the Governor to dispatch the Merlin sloop of war to England, to give intelligence of the danger. Mr. Robison and Mr. Harrison obtained leave to return in the Merlin, and sailed on the 28th, having been but a few days in Jamaica. This voyage was an epitome of all the disasters, short of shipwreck, to which seafaring men are exposed. They experienced a continuation of the most tempestuous weather, and the most contrary winds, from the moment they quitted the Bahamas till they arrived at Spithead. To add to their distress, the ship sprung a great leak, 300 leagues from any land, and it required the utmost skill and exertion to keep her from sinking. In a terrible gale, on March 14, their rudder broke in two: so that they could no longer keep the ship's head to the wind; and if the gale had not speedily moderated, they must inevitably have perished. When the voyage was near a conclusion, and they were congratulating themselves on the end of their troubles, the ship was found to be on fire, and the flames were extinguished with great difficulty. They reached Portsmouth on March 26; and on April 2 the mean noon by the watch was found to be at $11^h\ 51'\ 31\frac{1}{2}''$; and, making correction for the error and rate, this amounted to $11^h\ 58'\ 6\frac{1}{2}''$; so that the whole error, from the first setting sail, was only $1'\ 53\frac{1}{2}''$, which, in the latitude of Portsmouth, would not amount to an error, in distance, of 20 miles.

When Mr. Robison undertook the voyage to Jamaica, he made no stipulation for any remuneration, and Lord Anson assured him

that he should have no reason to repent the confidence which he placed in the Board. But when on his return he came to look for the reward, to which the success and trouble of the undertaking certainly entitled him, he soon found that he had greatly erred in leaving himself so much at the mercy of unforeseen contingencies. Lord Anson was ill of the disease of which he died, and was not in a condition to attend to business. Admiral Knowles was disgusted with the Admiralty, and with the Ministry, by which he thought himself ill-used; so that Mr. Robison had nothing to look for from personal kindness, and could trust only to the justice and moderation of his claims. These were of little advantage to him; for such was the inattention of the Lords of the Admiralty, and the Members of the Board of Longitude, that he could not obtain access to any of them, nor even receive from them any answer to his memorials.

The picture which his letters to his father present, at this time, is that of a mind suffering severely from unworthy treatment, where it was least suspected. Men in office do not reflect, while they are busy about the concerns of nations, how much evil may be done by their neglect to do justice to an individual. They may be extinguishing the fire of genius, thrusting down merit below the level it should rise to, or prematurely surrounding the mind of a young man with a fence of suspicion and distrust worse than the evils which it proposes to avert. Like other kinds of injustice, this may, however, meet with its punishment; though the victim of unmerited neglect may remain for ever obscure, and his sufferings for ever unknown, he may also emerge from obscurity, and the treatment he has met with may meet the eye of the public. It is probable that the member of these Boards most conspicuous for rank or for science would not have been above some feeling of regret if he had learnt that the young man whose petitions he disregarded was to become the ornament of his country, and the ill treatment he then met with a material fact in the history of his life.

But though we must condemn the neglect of which Mr. Robison had so much reason to complain, we do by no means regret that the recompense which he or his friends had in view was not actually conferred on him. This was no other than an appointment to the place of a purser in a ship of war; a sort of preferment which, to a man of the genius, information, and accomplishment, of Mr. Robison, must have turned out rather as a punishment than a reward. It was, however, the object which, by the advice of Sir Charles Knowles, he now aspired to; and indeed he had done so ever after his first voyage in the Royal William; for it appears that he had wished to be made Purser to the Peregrine at the time when Lieut. Knowles was appointed to the command of that ship, though, considering its smallness, the situation could have been attended with little emolument.*

* It is, however, true that the place of Purser was afterwards offered to Mr.

Thus disappointed in his hopes, Mr. Robison resolved on returning to Glasgow, in order to qualify himself for entering into the church. Indeed, the idea of prosecuting his original destination seems often to have occurred to him, even when his views appeared to have a very different direction. When he left the Royal William, in 1761, he was not without serious intentions of resuming the study of theology. This appears both from a letter he wrote to his father about that time, and from one which he himself received from young Knowles, who rallies him on his new profession, and on the singularity of having acquired a taste for theological studies in the ward-room of a man of war. When he undertook the voyage to Jamaica he would have wished to have had the patronage of his employers for obtaining some ecclesiastical preferment rather than naval; and only agreed to the latter as it lay more in the way of the Board of Longitude to help one to promotion in the navy than in the church. It appears that he had never ceased to express to Dr. Blair a desire of assuming the clerical character; and he actually had from that Gentleman the offer of a curacy in a living of his own, to which, however, the emolument annexed was so small that, after consultation with his father, he declined accepting of it.

But however Mr. Robison's views may have varied, to one object he steadily adhered, viz. the cultivation of science, and the acquisition of whatever knowledge the situations he was placed in brought within his reach.

He returned, therefore, to Glasgow; and a man whose object was the prosecution of science could not arrive at any place in a more auspicious moment, as that city was about to give birth to two of the greatest improvements which in the 18th century have distinguished the progress either of the sciences or the arts. The one of these was the discovery of latent heat, by the late Dr. Black; the other was the invention of what may be properly called a new steam-engine, by Mr. Watt. The former of these eminent men was then the Lecturer on Chemistry in the University, and had just been led, by a train of most ingeniously contrived experiments, to the knowledge of a principle which seemed to promise better for an explanation of the process which takes place when heat is communicated to bodies than any thing yet known in chemistry, viz. that when water passes from a solid to a fluid state, as much of its heat disappears as would have raised its temperature, had it remained solid, 140° higher than that which it actually possesses. Mr. Robison was already known to Dr. Black, having been introduced to him before he left Glasgow; but at that time he had not studied

Robison, but such a one as he could have no temptation to accept. In 1763, when Lord Sandwich was First Lord of the Admiralty, his solicitations were so far listened to that he was appointed to the Aurora, of 40 guns, then on the stocks. As the ship must be long of being in commission, and the pay of the Purser, in the mean time, very inconsiderable, Mr. Robison declined accepting this appointment.

chemistry, to which, however, he was now bending his attention. He had the advantage of being initiated in it by the author of the discovery just mentioned, and the new views struck out by his master did not fail to interest him in a study which from that time came to occupy a new place in physical science.

Mechanics had always been his favourite pursuit, and his turn to whatsoever was connected with it had brought him to be acquainted with Mr. Watt before 1758, when he left the University. Mr. Watt, who at that time exercised the profession of a mathematical instrument maker, was employed in fitting up the astronomical instruments bequeathed to the Observatory by the late Dr. Macfarlane, of Jamaica. Mr. Robison, on his return, found him still residing in Glasgow, and exercising the same profession, and their former intimacy was naturally renewed. In 1764 an occurrence such as to an ordinary man would have been of no value, gave rise to the improvement of the steam-engine. A model of the common engine, Newcomen's, which belonged to the Natural Philosophy Class, was put into Mr. Watt's hands in order to be repaired. As the model worked faster than the large engines, it was found impossible to supply it with steam, and it was in the attempt to obviate this difficulty, and in trying to produce a more perfect vacuum, that the idea of condensing the steam in a separate vessel first occurred to him. At the same time, by a curious coincidence, his experiments led him to conclusions concerning the great quantity of heat contained in steam, that were only to be explained on the principle of latent heat. Mr. Robison lived in a state of great intimacy with Mr. Watt, and was so much acquainted with the first steps of this invention that his evidence on the subject of the originality of it was afterwards of great use in ascertaining the justness of his claim.

There could not be a better school for philosophical invention than Mr. Robison enjoyed at this time, and accordingly he used always to say that it was not till his second residence at Glasgow that he applied to study with his whole mind.

Dr. Black was elected Professor of Chemistry in the University of Edinburgh in the summer of 1766; and on leaving Glasgow recommended Mr. Robison as his successor. He was accordingly made choice of, and began his first course of chemical lectures in October, 1766. He was appointed for one year only; but his success assured his continuance without any other limit than such as depended on himself.

He had also the charge of the education of the late Mr. Macdowal, of Garthland, and of Mr. Charles Knowles, a son of the Admiral. But of the particulars, during four years, about this time, I have been able to obtain little information.

The friendship of Admiral Knowles had been all along exerted toward Mr. Robison with an extraordinary degree of zeal and assiduity, and was now the means of procuring for him a very unlooked-for preferment, which removed him from his academical

duties at Glasgow. The Empress of Russia, convinced of the importance of placing her marine on the best footing, made an application to the Government of this country for permission to engage in her service some of the most able and experienced of our naval officers, to whom she might entrust both the contrivance and the execution of the intended reformation. The request was agreed to, and the person recommended was Admiral Sir Charles Knowles, who had long applied with great diligence to the study of naval architecture, as well as to that of every branch of his profession, and who about 50 years before had been sent to Portugal on a similar mission. A proceeding so free from that jealousy which often marks the conduct of great nations no less than the dealings of the most obscure corporations, is particularly deserving of praise. From the first moment that this offer was made to the Admiral, he communicated it to Mr. Robison, whom he wished to engage as his secretary, and to whom, as he says in his letters, he looked for much assistance in the duty he was about to undertake. A very handsome appointment was made for Mr. Robison, and in the end of December, 1770, he set out with Sir Charles and his family on the journey to St. Petersburgh, over land.

Admiral Knowles held the office of President of the Board of Admiralty; and his intention was that Mr. Robison should have the place of Secretary. The Russian Board, however, being constituted more on the plan of the French than the English, there was no place corresponding to that of our Secretary of the Admiralty. Mr. Robison continued, therefore, in the character of Private Secretary to the Admiral.

During the first year of the Admiral's residence in Russia, and for the greater part of the second, Mr. Robison remained with him, employed in forming and digesting a plan for improving the methods of building, rigging, and navigating, the Russian ships of war, and for reforming, of consequence, the whole detail of the operations in the naval arsenals of that empire.

These innovations, however, met with more resistance than either the Admiral or his Secretary had permitted themselves to suppose. The work of reform, conducted by a foreigner, even when he is supported by despotic power, must proceed but slowly; jealousy, pride, and self-interest, will continually counteract the plans of improvement, and by their vigilance and unceasing activity will never wholly fail of success. All this was experienced by Admiral Knowles; yet there is no doubt that material advantages were derived by the Russian navy from the new system which he was enabled partially to introduce.

Mr. Robison, from his first arrival at St. Petersburgh, had applied with great diligence to the study of the Russian language, and had made himself so much master of it as to speak and write it with considerable facility. In summer 1772, a vacancy happening in the mathematical chair attached to the Imperial Sea Cadet Corps of Nobles, at Cronstadt, Mr. Robison was solicited to accept of

that office. His nautical and mathematical knowledge qualified him singularly for the duties of it, and his proficiency in the Russian language removed the only objection that could possibly be proposed. When he accepted of the appointment the salary of his predecessor was doubled, and the rank of Colonel was given him. Besides delivering his lectures as Professor, he officiated also as inspector of the above corps, in the room of Gen. Politika, who had retired, or been sent to his estates in the Ukraine.

The lectures which he gave were very much admired, and could not fail to be of the greatest use to his pupils. Few men understood so well the theory and the practice of the arts they profess to teach; few had enjoyed the same opportunities of seeing the mathematical rules of artillery and navigation carried into effect on so great a scale. To his own countrymen resident at St. Petersburgh Mr. Robison was an object of no less affection than admiration.

In 1773 the death of Dr. Russell produced a vacancy in the Natural Philosophy Chair of the University of Edinburgh. Principal Robertson, who was ever so attentive to the welfare of the University over which he presided, though not personally acquainted with Mr. Robison, yet knowing his character, had no doubt of recommending him to the Patrons of the University, who, on their part, with no less disinterestedness, listened to his recommendation, and Mr. Robison was accordingly elected. It is said that when the news of this appointment reached him, he at first hesitated about the acceptance of it, principally from the fear of appearing insensible to the kindness and favour which he had experienced from the Russian Government. The moment, too, when it was known that this invitation had been given him, further offers of emolument and preferment were made him by that Government, of such a kind as it was supposed he could not possibly resist. At length he determined, and no doubt wisely, however splendid the prospects held out to him might be, to accept of a situation that would fix him permanently in his native country. He therefore declined the offers of the Empress of Russia, and in June, 1774, sailed from Cronstadt for Leith, followed, as one of those friends he left behind in Russia has expressed it, by the regrets, and accompanied by the warmest good wishes, not only of all who had shared in his friendship, but of all to whom he was known. The Empress was so far from being offended with his determination, however much she wished to prevent it, that she settled a pension on him, accompanied with a request that he would receive under his care two or three of the young cadets who were to be selected in succession.

Mr. Robison was admitted at Edinburgh on Sept. 16, 1774, and gave his first course of lectures in the winter following. The person to whom he succeeded had been very eminent and very useful in his profession. He possessed a great deal of ingenuity, and much knowledge, in all the branches of physical science. Without perhaps being very deeply versed in the higher parts of the mathematics, he had much more knowledge of them than is re-

quisite for explaining the elements of natural philosophy. His views in the latter were sound, often original, and always explained with great clearness and simplicity. The mathematical and experimental parts were so happily combined, that his lectures communicated not only an excellent view of the principles of the science, but much practical knowledge concerning the means by which those principles are embodied in matter, and made palpable to sense.

Mr. Robison, who now succeeded to this chair, had also talents and acquirements of a very high order. The scenes of active life in which he had been early engaged, and in which he had seen the great operations of the nautical and the military art, had been followed or accompanied with much study, so that a thorough knowledge of the principles, as well as the practice, of those arts had been acquired. His knowledge of the mathematics was accurate and extensive, and included, what was at that time rare in this country, a considerable familiarity with the discoveries and inventions of the foreign mathematicians.

In the general outline of his course he did not, however, deviate materially from that which had been sketched by his predecessors, except, I think, in one point of arrangement, by which he passed from dynamics immediately to physical astronomy. The sciences of mechanics, hydrodynamics, astronomy, and optics, together with electricity and magnetism, were the subjects which his lectures embraced. These were given with great fluency and precision of language, and with the introduction of a good deal of mathematical demonstration. His manner was grave and dignified; his views, always ingenious and comprehensive, were full of information, and never more interesting and instructive than when they touched on the history of science. His lectures, however, were often complained of, as difficult and hard to be followed; and this did not, in my opinion, arise from the depth of the mathematical demonstrations, as was sometimes said, but rather from the rapidity of his discourse, which was in general beyond the rate at which accu ate reasoning can be easily followed. The singular facility of his own apprehension made him judge too favourably of the same power in others. To understand his lectures completely was, on account of the rapidity and the uniform flow of his discourse, not a very easy task, even for men tolerably familiar with the subject. On this account his lectures were less popular than might have been expected from such a combination of rare talents as the author of them possessed. This was assisted by the small number of experiments he introduced, and a view that he took of natural philosophy which left but a very subordinate place for them to occupy. An experiment, he would very truly observe, does not establish a general proposition, and never can do more than prove a particular fact. Hence he inferred, or seemed to infer, that they are of no great use in establishing the principles of science. This seems an erroneous view. An experiment does but prove a particular fact; but by doing so in a great number of cases it affords the means of

discovering the general principle which is common to all these facts. Even a single experiment may be sufficient to prove a very general fact. When a guinea and a feather, let fall from the top of an exhausted receiver, descend to the bottom of it in the same time, it is very true that this only proves the fact of the equal acceleration of falling bodies in the case of the two substances just named; but who doubts that the conclusion extends to all different degrees of weight, and that the uniform acceleration of falling bodies of every kind may safely be inferred.

A society for the cultivation of literature and science had existed in Edinburgh ever since the year 1739, when, by the advice, and under the direction of Mr. Maclaurin, an association, formed some years before for the improvement of medicine and surgery, enlarged its plan, and assumed the name of the Philosophical Society. This Society, which had at different times reckoned among its members some of the first men of whom this country can boast, had published three volumes of memoirs, under the title of Physical and Literary Essays; the last in 1756, from which time the Society had languished, and its meetings had become less frequent. At the time I am now speaking of it was beginning to revive, and its tendency to do so was not diminished by the acquisition of Mr. Robison, who became a member of it soon after his arrival. It had often occurred that a more regular form, and an incorporation by royal charter, might give more steadiness and vigour to the exertions of this learned body. In 1783, accordingly, under the auspices of the late excellent Principal of this University, a royal charter was obtained, appointing certain persons named in it as a new society, which, as its first act, united to itself the whole of the Philosophical.

Professor Robison, one of those named in the original charter, was immediately appointed Secretary, and continued to discharge the duties of that office till prevented by the state of his health several years after.

The first volume of the Transactions of this Society contains the first paper which Professor Robison submitted to the public, a Determination of the Orbit and the Motion of the Georgium Sidus directly from Observations, read in March, 1786. This planet had been observed by Dr. Herschell on March 13, 1781, and was the first in the long list of discoveries by which that excellent observer has for so many years continued to enrich the science of astronomy. Its great distance from the sun, and the slowness of its angular motion, which last amounts to little more than four degrees from one opposition to the next, made it difficult to determine its orbit with tolerable accuracy, from an arch which did not yet exceed an eighteenth part of the whole orbit. This was an inconvenience which time would remedy; but impatience to arrive even at such an approximation as the facts known will afford is natural in such cases, and Professor Robison, as well as several other mathematicians, were not afraid to attempt the problem, even

in this imperfect state of the data. It is well known that the observations which best serve the purpose of determining the orbit of a planet are those made at its oppositions to the sun, when an observer in the earth and in the sun would refer the planet to the same point in the starry heavens, or when, in the language of astronomers, its heliocentric and geocentric places coincide. Of these oppositions in the case of this planet there were yet only four which had been actually observed. Dr. Herschell had, however, discovered the planet soon after the opposition of 1781 was passed, and though of course that opposition was not seen, yet from the observations that were made so soon after, Professor Robison thought he could deduce the time with sufficient accuracy. The opposition of the winter 1786 he observed himself; for though there was unfortunately no observatory at Edinburgh, he endeavoured to supply that defect on the present occasion by a very simple apparatus, viz. a telescope on an equatorial stand, which served to compare the right ascension and declination of the planet with those of some known stars which it happened to be near. His general solution of the problem is very deserving of praise; and though the method pursued is in its principle the same with all those which ever since the time of Kepler have been employed for finding the elements of a planetary orbit, it appears here in a very simple form, the construction being wholly geometrical, and easily understood. The elements, as he found them, are not very different from those that have since been determined from more numerous and more accurate observations.

When Dr. Herschell first made known this most distant of the planets, many astronomers believed that they had discovered the source of those disturbances in our system which had not yet been explained. Professor Robison was of this number; for he tells us in the beginning of his paper that he had long thought that the irregularities in the motion of Jupiter and Saturn, which had not been explained by the mutual gravitation of the known planets, were to be accounted for by the action of planets of considerable magnitude, beyond the orbit of Saturn. Subsequent inquiry, however, has not verified this conjecture; the irregularities of Jupiter and Saturn have since been fully explained, and are known to arise chiefly from their action on one another, a very small part only being owing to that of the Georgium Sidus, or of any of the other planets.

The next publication of Professor Robison was a paper in the second volume of the same Transactions, On the Motion of Light, as affected by Refracting and Reflecting Substances, which are themselves in Motion.*

The phenomena of the aberration of the fixed stars are well known to depend on the velocity of the earth's motion combined with the velocity of light; the quantity of the aberration, when all

* Edinburgh Transactions, vol. ii. p. 83.

other things are given, being directly as the first, and inversely as the second. It is not, however, the general or the medium velocity with which light traverses space, but it is the particular velocity with which it traverses the tube of the telescope, that determines the quantity of this aberration. Were it possible, therefore, to increase or diminish that velocity, the aberration would be diminished in the first case, and increased in the second. But according to the principles now generally received in optics, the velocity of light is increased when it traverses a denser medium, or one in which the refraction is greater; and therefore were the tube of a telescope to be filled with water instead of air, the aberration would be diminished. Professor Robison, and his friend Mr. Wilson, Professor of Astronomy at Glasgow, had speculated much on this subject, and made many attempts to obtain a water telescope, but hitherto without effect. A paper of Boscovich on the same subject seemed to suggest some new views, that might render the experiment more easy to be made. That philosopher maintained that in ascertaining the effect of a water telescope on the motion of light, the observation of celestial objects might be dispensed with, and that of terrestrial substituted in its place. He argued that while light moves with an uniform velocity, the telescope must be directed, not to the point of space which the object occupied when the particle was sent off which is entering the telescope, but to a point advanced before it by a space just equal to that which both the object and the observer have passed over in the time in which the particle has passed from the object to the eye. It is therefore directed exactly to the place which the object is in when the light from it enters the eye. If, therefore, the ray, on entering the telescope, is made to move faster than it did before, the telescope must not be inclined so much, and the apparent place of the object will fall behind its true place. If the ray is retarded on entering the water, the contrary must happen. Hence a number of very unexpected phenomena would result, affording, without having recourse to the heavenly bodies, a direct proof of the motion of the earth in its orbit, as well as a resolution of the question whether light is accelerated or retarded on passing from a rarer to a denser medium.*

On this reasoning Professor Robison has very well remarked that it would be just if the light, on entering the water telescope, had only its velocity changed, and not its direction. But this is not the case; for the ray that is to go down the axis of the telescope is not perpendicular to the surface of the fluid; it makes an angle with it, depending on the aberration, and therefore in some cases less by 20″ than a right angle. On this account the effect is not produced which Boscovich's reasonings lead us to expect.

The sequel of the paper is also full of ingenious remarks.

* Boscovich, Opera Math. tom. ii. opusc. 3.

(To be continued.)

ARTICLE II.

On the Stability of Vessels. By Col. Beaufoy, F.R.S. With Two Plates.

(To Dr. Thomson.)

MY DEAR SIR, *Bushey Heath, Dec.* 22, 1815.

THAT part of hydrostatics, which treats on the stability of floating bodies, naturally interests the curiosity of most persons in a maritime country like Great Britain, and excites the desire of many to become acquainted with the law which regulates their equilibrium. Being one of those who are attached to this interesting subject, I take the liberty of laying before you a series of experiments, which, should they prove instrumental in throwing new light on naval architecture, or in improving the construction of vessels, will amply recompense the trouble they cost, in the hope that the time and expense bestowed on them have not been uselessly employed.

I remain, my dear Sir, yours very sincerely,

MARK BEAUFOY.

Experiments to verify the Theorems on Stability, particularly M. Bouguer's, with a Description and Drawing of the Apparatus with which they were made, and some Remarks on the Formation of Vessels.

THE principal object in making these experiments was to bring to the test of experiment the different theorems of various writers on naval architecture, particularly those of M. Bouguer for calculating the stability of variously shaped floating bodies. This Gentleman founds his theory on the supposition that the angles of inclination assumed by floating bodies are evanescent, which in a practical sense may be regarded as angles which are very small. But as vessels at sea frequently, by the pressure of their sails, as well as by the action of the waves, make very large angles with the horizon or surface of the water, before implicit confidence be placed in any theory, it is but prudent to submit it to the test of experiment. The first theorem to be examined is,

That the stability is in proportion to the squares of the areas of the horizontal surfaces or sections.

2. That the height of the metacentre of the parallelopipedon above the centre of gravity of the displaced water is found by dividing the square of half the breadth of the parallelopipedon by three times the draft of water, that is, the depth to which the parallelopipedon is immersed in the fluid.

3. That the metacentre of a right angle triangle is elevated as much above the line of floatation as the centre of gravity of the displaced water is depressed below the surface.

4. That the metacentre of a semi-ellipsis above its line of floata-

Fig. 1.

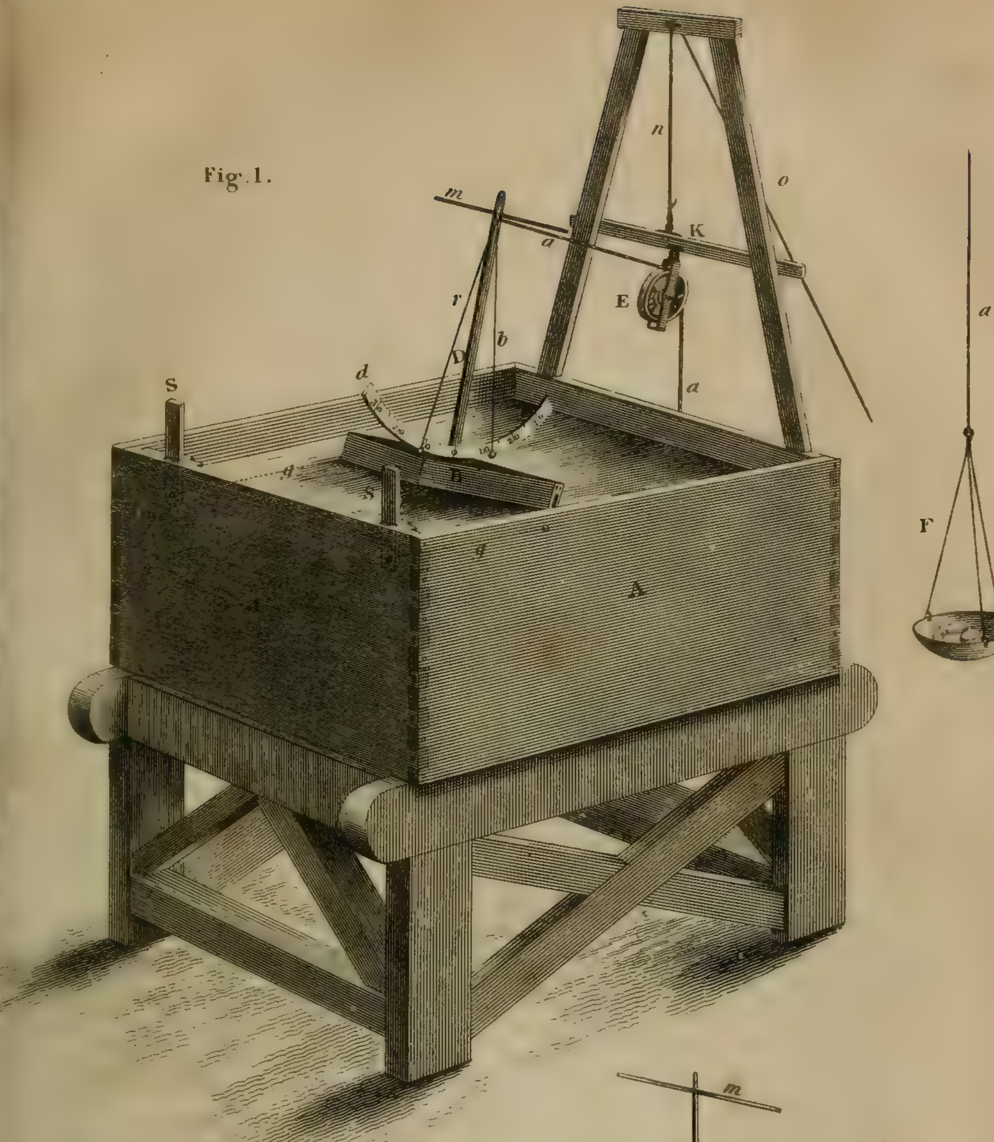
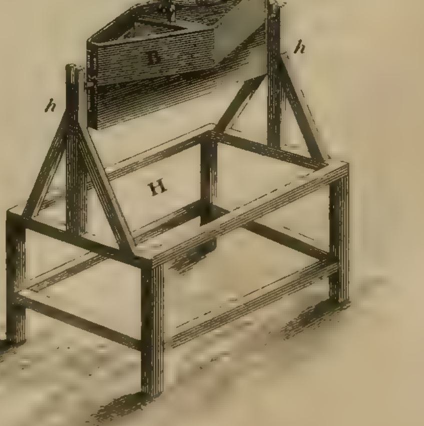


Fig. 3.

Fig. 2.



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Engraved for Dr Thomson's Annals for Baldwin Cradock & Joy Paternoster Row March 1 1816

tion is found by subtracting the square of the draft of water from the square of half the breadth, and then dividing three-eighths of the remainder by the draft of water.

5. That the stability of vessels is augmented in proportion to the cubes of the breadths, provided that the centres of gravity of the vessel and of the displaced fluid coincide.

6. That vessels having the same length and breadth, but different drafts of water, have equal stability when their centre of gravity, and the centre of gravity of the displaced water, are in the same point.

7. That the altitude of the metacentre in different vessels above the centre of gravity of the displaced water is proportional to the squares of the breadths.

The apparatus for making these experiments was simple, as appears from the perspective view (Plate XLIV. Fig. 1) of the whole together. In this A A represents a cistern filled with water, and mounted to a convenient height upon framed legs; B, a model on which the experiment was tried, by attaching a fine line, *a a*, to the top of the mast, D, and conducting it over a pulley, E. A scale, F, is suspended to the end of the line for the reception of the weights. These cause the model to incline, as the figure shows; and the degree of inclination of the mast from the perpendicular is shown by the plumb-line, *b*, upon a divided arch, *d*. To prevent the body being drawn away towards the pulley, E, by the draft of the line, *a*, it is retained by two small lines (shown dotted at *g*, *g*), which are made fast to sliders, *s*, *s*, at the side of the cistern, and have hooks at the opposite ends, which take hold of pins projecting from the stem and stern of the model, B; and these are previously adjusted, so that the centre of gravity of the model will be found in a line between them. The manner of making this adjustment is shown in Fig. 2, which represents a frame of wood, H, supporting two small uprights, *h*, *h*. These have pieces of brass plate at the upper ends, with notches to receive the pins or pivots of the model, B. These pivots are fitted into grooves in two pieces of brass plate attached to the ends of the model. One of these slips of brass is shown separately at Fig. 3, where *k* is the pivot, and *l* a screw tapped into the brass slide to which the pivot is fixed, and passing through the same groove by means of this screw, the pivot *k* can be fastened at any part of the groove, and raised or lowered. The ballast is then raised or lowered till the model will barely rest on the pivots without overturning, as shown in Fig. 2. It is necessary, in order to know exactly what weight is applied to the top of the mast, D, that the line, *a*, draw in a direction at right angles thereto. To ascertain this, a ruler, *m*, is fixed upon the top of the mast, and the pulley, E, is attached to a cross rail, H, which applies against the uprights, and is suspended by a line, *n*, which passes over a pulley, and is made fast to a cleat, *o*. By this means the pulley, E, can at pleasure be raised or lowered until the direction of the line, *a*, corresponds with the ruler, *m*. The manner of conducting

the experiments with this apparatus is as follows. The cistern is filled with water up to a certain mark; and the model being put in, loaded with ballast, the water is added or decreased till the edge of the gunwhale is exactly on a level with the edge of the cistern, as ascertained by looking across it, or applying a straight ruler. The plumb-line, *d*, cutting the zero of the divided arch, shows the vessel to be upright. In this state the model is ready for making the experiments. The hooks of the two strings, *g*, *g*, attached to the pivots, and the two sliders, *s*, *s*, are raised or lowered to make the strings, *g*, *g*, horizontal in the water. Weights being now put into the scale, F, will show what weight is requisite to incline the model. The pulley, E, being raised or lowered by the line *n*, as is found necessary to make the line, *a*, draw parallel to the ruler, *m*, or perpendicular to the mast. The inclination of the mast is shown by the plumb-line, *b*, cutting the divisions of the arch, *d*; but to counteract the weight of the plummet, *b*, which tends to incline the mast, another counterbalancing plummet and line, *r*, is applied on the opposite side of the model. For this purpose holes are made in the arch, *d*, at every division, and a peg is put in at the division opposite to that which is cut by the plumb-line. The experiment is tried with different weights, to produce the several inclinations at every 5°, until 30° from the perpendicular; and to verify the experiment, the model is changed end for end, the strings, *g*, *g*, being hooked on the pivots at the opposite ends. In this way the series of trials are made on the opposite side, by which means, if there is any difference in the two sides, or in the ballast, it will be detected, and allowed for by taking the mean of the different trials.

Example with Model I. (Plate XLV.)—*Exper.* 1.

The total depth of the model being 7·5 inches, the height of the centre of gravity is subtracted in each experiment, and the length of the mast is afterwards added, which gives the length of lever at which the weight is applied to produce the various inclinations of 5°, 10°, 15°, 20°, 25°. The centre of gravity in Exper. 1 being situated two inches above the bottom of the model, 2 oz. 10 dr. inclined it 5°. The model was turned end for end; and to incline it, the same angle required 3 oz. 5 dr. The mean of both is 2 oz. 15 dr., which is considered as the true power. This number (2 oz. 15 dr.) being reduced to the decimals of an oz., gives 2·9687 oz., and is set down on the right hand in Table I. It is evident that the effort of the water to restore the vessel to its original vertical position is exactly equal to the inclining power. If, therefore, the momentum of the effort to incline the vessel be divided by the weight of the displaced fluid, the quotient will be the length of lever on which the water acts.

Exper. 1.—Model 1 inclined 30°; inclining power, 20·3125 oz. length of lever, 24·98 inches; and weight of displaced fluid, 559·06 $\frac{20{\cdot}312 \times 24{\cdot}98}{559{\cdot}06} = 0{\cdot}90758$ parts of an inch, the length of

lever on which the fluid acts, which, for the sake of distinction, call x. Then, to find the greatest height, the centre of gravity of the model can be raised above the bottom without oversetting (called by the French writers on naval architecture the *metacentre*), as $30^\circ : x$ or ·90758 :: radius : 1·8152, or the height of the metacentre above the centre of gravity, which call y Then 2 inches, the height of the model's centre of gravity, added to 1·8152, the sum 3·8152 is the altitude of the metacentre, according to this experiment, above the bottom of the model. By the same process the height of the metacentre was obtained, when the experiment was made at 10°, 15°, 20°, 25°. To ascertain more exactly the height of the metacentre, the centre of gravity of the figure was raised. In Exper. 2, the height of the centre of gravity was raised to 2·25 inches above the under side of the figure; and in Exper. 3, to 3 inches. By these two, the accuracy of the first experiment was corroborated. Three heights are thus obtained. But from the unavoidable inaccuracies to which experiments are liable, these numbers differ; therefore take the mean of the heights of the metacentre thus determined. But as these means, when compared, are found neither to increase nor decrease in a regular manner, take the differences between each set of experiments; and then take the mean of all the differences, and examine which of the differences most nearly coincides with the mean difference. The two experiments whose difference is the nearest to the mean difference are to be considered as the best experiments. Then, by adding and subtracting the mean common difference, the whole will be brought into a regular series, and the irregularities corrected. The following example will more clearly explain the process. In Table 3, are set down the various heights of the metacentre at every angle of inclination, as determined by Exper. 1, 2, and 3; and the fourth horizontal line contains the mean results. In the eighth right hand vertical column are placed the differences of the mean heights. The mean of the five differences is ·0538. The nearest difference to this number is ·0584, which is the difference between the mean height of the metacentre at 15° and 20°. By taking the mean of the experiments at 15° and 20°, 3·5995 — 3·6579 = 3·6287; and adding and subtracting half of ·0538 (which is ·0269) to 3·6287, the numbers 3·6556 and 3·6018 are obtained, which are the corrected heights of the metacentre. Then by adding ·0538 to 3·6556, and subtracting the same from 3·6018, the corrected altitudes of the metacentre are obtained, which are set down in the last column on the right. By subtracting the altitude of the model's centre of gravity from these numbers, a more accurate value of y is obtained. (See Table 4.) Then with the value of y, and the angle of inclination of the model, the length of x is obtained. This number multiplied by 559·06, the weight of the displaced water, gives the momentum of the stability. This number divided by the length of lever which inclined the model, gives those numbers called the regular series; and according to the agreement or non-agreement

of these numbers or weights with the experimented number or weight which inclined the model, a judgment may be formed of the accuracy of the experiments. See example in the experiments with model 1, in the first page of the tables. Having shown the manner of making the experiments, I shall proceed to examine the first theorem.

To find the momentum of the section of water of models 1, 2, 3, 5, and 16, the momentums, by experiments, of these models were compared together. These bodies had the same length, breadth, and depth; except model 16, which was only half the length; and each of them were immersed in the water 4·5 inches, or half their respective breadths. The centre of gravity of 1, 2, 3, was raised 2·25 inches above the bottom of the model; and of models 5 and 16, 3 inches; or at the same point as the centre of gravity of the displaced fluid. Suppose the stability to be as some power of the area of the horizontal section, then $A^m : a^m :: S : s$; A representing the area of the horizontal section of parallelopipedon equal to 215; and a, the area of the ellipsis equal to 168; S being the momentum of the parallelopipedon (when the inclination was 5°) equal to 60·624; and s, the momentum of the ellipsis (at the same angle of inclination) equal to 36·47. The required power, or exponent, or the value, of m, is $= \frac{\text{Log. } S - \text{Log. } s}{\text{Log. } A - \text{Log. } a}$.

5°	10°	15°	20°	25°	30°	
2·0605,	2·0350,	2·0115,	1·9903,	1·9705,	1·9529;	mean value of exp. m, 2·0030.

In the next place compare the rhombus with the parallelopipedon. It is evident the former is precisely half the area of the latter; and by comparing the experiments, nearly the same result is obtained as with the ellipsis; namely, when the centre of gravity of the body is situated in the same point as the centre of gravity of the displaced fluid, the stability is as the square of the surface, the momentum being nearly $\frac{1}{4}$ of the momentum of the parallelopipedon, as appears by the subjoined table:—

5°	10°	15°	20°	25°	30°	
2·0082,	2·0038,	1·9995,	1·9955,	1·9645,	1·9952;	mean value of exp. m, 1·9945.

Model 16 being but half the length of model 5, the momentum of model 16 must be doubled when compared with model 5. The value of m is as follows:—

5°	10°	15°	20°	25°	30°	
2·1770,	2·1318,	2·0846,	2·0362,	1·9862,	1·9334;	mean value of exp. m, 2·0582.

The momentum of model 5 is greater or less, as the centre of gravity falls short or exceeds 3 inches above the bottom of the model. From these results of the value of m, it appears extremely probable that the momentum of all figures is in proportion to the squares of the area. This conclusion from experiments corroborates the first theorem.

By examining the experiments, the result of those with model 1 at an angle rather less than 30° agrees with the second theorem. The result of those with model 4 show the theorem in excess, or the angle of inclination must be more than 30° to agree with it. Result of experiments with model 8 nearly agrees with the second theorem at an angle of 25°; and result with model 9 agrees at an angle rather less than 20°.

By experiments with model 16, it appears that the third theorem is erroneous, because in no instance could the metacentre be elevated so high.

Experiments with models 11 and 12 show the fourth theorem will not coincide with experiment, unless the angle be more than 30°. Experiments with model 13 nearly agree with theorem, but the experiments are still minus. Model 14, at an angle of 15°, nearly agrees with the theorem; and experiments with model 15, at an angle between 10° and 15°, agree with the theorem.

Theorem Fifth.—To ascertain the law of stability, or how much more sail a vessel will carry by augmenting the breadth, compare parallelopipedons 1 and 4, 1 and 8, 8 and 9, 7 and 9, also models 16 and 17, 17 and 18, 16 and 18. Suppose the stability to increase or decrease, according to some power of the breadth, B b represent the breadth; S s, the momentum of the stability, as $B^m : b^m :: S : s$; the exponent m is $= \frac{\text{Log. S} - \text{Log. } s}{\text{Log. B} - \text{Log. } b}$.

5°	10°	15°	20°	25°	30°		
3·470	3·458	3·447	3·437	3·428	3·420	1 and 4,	Mean 3·443 of the various valne of m.
4·035	3·637	3·383	3·280	3·321	3·439	1 and 8,	3·516
3·739	3·596	3·482	3·428	3·472	3·610	1 and 9,	3·554
3·411	3·551	3·589	3·590	3·639	3·800	7 and 9,	3·586
3·595	3·233	2·993	2·901	2·967	3·238	16 and 17,	3·154
3·189	2·980	2·842	2·789	2·830	2·976	16 and 18,	2·936
2·739	2·701	2·675	2·663	2·671	2·698	17 and 18,	2·691

By the above table it appears that the stability of a parallelopipedon increases in a greater proportion than the cubes of the breadth, and that the stability of models 16 and 17 increased in a greater proportion than the cubes; but the stability of models 16 and 18 was less than the cubes; and the stability of models 17 and 18 was still less than that of models 16 and 18.

The truth of the sixth theorem is corroborated by referring to the experiments made with model 16 at an angle of nearly 20°, the centre of gravity being at 3 inches; and to experiments with model 20 at an angle of nearly 20°, the centre of gravity being at 6 inches. The stability of model 16 is greater than the stability of model 20 from 15° to 5°, but less from 20° to 30°.

That theorem seventh does not agree with experiments, will be seen by experiments made with models 1 and 8, 1 and 9, 8 and 9,

16 and 17, 17 and 18, 16 and 18. Compare the experiments made with models 1 and 8 at 5°, $m = \frac{\text{Log. } 1{\cdot}702 - \text{Log. } 1{\cdot}244}{\text{Log. } 10 - \text{Log. } 8} = 2{\cdot}977.$

5°	10°	15°	20°	25°	30°		
2·977	2·579	2·350	2·222	2·243	2·380	1 and 8, Mean of the values of m,	2·455.
2·708	2·565	2·451	2·396	2·431	2·579	1 and 9, ———	2·522.
2·411	2·551	2·584	2·589	2·639	2·800	8 and 9, ———	2·596.
2·536	2·174	1·935	1·841	1·915	2·168	16 and 17, ———	2·095.
2·158	1·949	1·811	1·757	1·799	1·945	16 and 18, ———	1·903.
1·739	1·700	1·674	1·663	1·671	1·698	17 and 18, ———	1·691.

The vertical section of models 11, 12, 13, 14, 15, is an ellipsis, which is not a bad form for the midship bend of a large ship or vessel of burthen; and the experiments with these bodies were made for the purpose of ascertaining the result of increasing the breadth and diminishing the draft of water. By inspecting the tables, the following conclusions are drawn. By comparing the experiments with model 11 and model 13, it appears, in Exper. 1 and 2, when the centre of gravity of each model is placed at the same distance from the bottom, that model 11 had the greater stability; but by Exper. 3, it appears that when the centre of gravity of each model was raised to the surface of the water, model 13 had the advantage in stability; and as the centre of gravity rises, the stability of model 13 may exceed that of model 11 by more than any assignable quantity. With the view of deducing some practical advantages from these experiments, it will be necessary to fix the centre of gravity at some point, that in all probability may not be widely different in practice, and which is supposed to be at the surface of the water; previously observing, that a semicircular form is totally rejected from practice when the centre of gravity is near the water line; for although this shape has sufficient stability when the centre of gravity is low down, the stability becomes insensible when the centre of gravity coincides with the centre of the circle. In these experiments, it is true the metacentre falls short of this distance by $\frac{1}{10}$ of an inch. This deviation arises in a degree from the part above the water being a tangent to the curve instead of a continuation, which causes model 10 to rise out of the water as it inclines. Model 11 has its breadth increased precisely as much as model 13 has the draft of water diminished. By comparing those experiments together in which the centre of gravity is situated at the surface of the water, it is evident that model 13 has more stability than model 11. It had the advantage also, when the centre of gravity was situated at 3 inches above the bottom, or $\frac{1}{2}$ an inch below the surface of the water. Again, by comparing the experiments with models 12 and 14, the draft of water is diminished 2 inches; and it is remarkable the diminution of the depth has the advantage in stability over the increase of width, the centre of

gravity in both cases being at the water's surface, and the maximum is at an angle of inclination of 20°; but when the centre of gravity is situated $\frac{1}{2}$ an inch below the surface of the water, model 12 has the advantage from 15° and upwards, as appeared from experiments; when the draft of water was decreased 1 inch more, being reduced to 1·5 inch, the stability was increased but little. It is therefore probable that if the bottom of a boat be a semi-ellipsis, and the top sides, or the part above the water, be perpendicular to the horizon, or parallel to the plane of the masts, the maximum of stability, when the centre of gravity is at the load water line, is nearly $\frac{1}{6}$ of the beam. From whence it appears that great advantage accrues by reducing the depth rather than increasing the breadth of boats designed for shoal water; as the latter, under the limits above-mentioned, are not only stiffer, but lighter, and of course more easily transported from place to place. But as vessels with a shallow draft of water, unless very long, are leewardly under sail, I know of no contrivance to obviate this defect comparable to Admiral Schank's ingenious invention of the sliding keels.

Model 19 shows by experiments that if those parts of the vessel above the surface of the water incline inwards, the stability is diminished. In experiments with models 11, 12, and 13, the metacentre rises as the angle of inclination increases. In experiments with models 14, 15, 16, 17, 18, and 19, the metacentre lowers as the angle of inclination increases. In experiments with models 20, 21, 22, and in the compound figure, model 23, the metacentre is stationary.

From these experiments it is concluded that the draft of water should be $\frac{2}{5}$ of the breadth, exclusive of the keel, if the centre of gravity and the greatest breadth are at the surface of the water. This proportion would answer for men of war, for ships heavily rigged, or designed to carry part of the cargo on the deck; but should a ship be designed to carry heavy materials, it should be narrower and deeper, which would prevent its being laboursome at sea, and less likely to be dismasted; for broad and shallow vessels laden with heavy materials are liable to lose their masts, from the violent and jerking motion to which they must necessarily be exposed in agitated waters, as their centre of gravity is situated much beneath the surface of the water. If the greatest vertical section or midship-bend of a vessel be a parallelopipedon, it is evident, from experiments with model 9, that the draft of water, if the centre of gravity be at the surface of the water, must bear a less proportion than $\frac{9}{22}$ to the breadth: and it is worthy of remark that a vessel of the last-mentioned shape, when the depth to which it is immersed is small, will be sufficiently stiff to a certain point, and overturns suddenly if the angle of inclination be increased, which was the case with model 8. Immersed $2\frac{1}{4}$ inches, and its centre of gravity $4\frac{1}{2}$ inches, it had stability at 30°, but overset suddenly at 35°. The constructors of the Thames barges appear to be sensible of this, and make the vertical sections frustums of triangles, which

prevents their oversetting, when so light that the angle of the bottom, by the power of the sail, rises above the surface of the water. It is not perhaps generally known that a mere increase of length diminishes the resistance of the fluid. The subjoined table contains the resistance of a globe 13·54 inches in diameter; and the resistance to the same sphere when cut in halves, and lengthened by the introduction of a cylinder whose length was the same as the globe's diameter :—

Feet per second	1	2	3	4	5	6	7
Resistance to globe in lbs.	0·40	1·56	3·45	6·03	9·28	13·21	17·77
Ditto globe lengthened in lbs.	0·17	0·82	1·99	3·71	6·00	8·87	12·34

Feet per second	8	9	10	11	12	13	14
Resistance to globe in lbs.	22·98	28·80	35·25	42·31	49·98	58·26	67·15
Ditto globe lengthened in lbs.	16·40	21·07	26·36	32·28	38·82	46·08	54·01

Feet per second	15	16	17	18	19	20
Resistance to globe in lbs.	76·65	86·74	97·42	108·69	120·56	133·01
Ditto globe lengthened in lbs.	62·61	71·89	81·85	92·51	103·87	115·93

This experiment shows the advantage of length; and perhaps because the water by striking the bow or fore part is put into eddies and whirls, to which the additional length gives time to subside before the fluid closes in behind. I took some pains to inquire into the character (if I may use the expression) of a very long ship, called L'Invention. This vessel measured on the beam, or broadest part, 27 feet 6 inches; and on the lower deck 135 feet 5 inches. Capt. Padrift, the Commander, whose professional abilities are held in great estimation, spoke so highly of this ship's good qualities, that I am confirmed in the opinion of the advantage of length. In the first place, the stability is increased, provided the centre of gravity is not raised; and, secondly, the length gives an opportunity of spreading greater extent of canvass low down, which is tantamount to an increase of stability, because taunt masts have more effect to overset, and press the head downwards, than to increase the velocity; thirdly, the vessel sails more smoothly in agitated water, and steers better. I am more convinced of the advantages to be derived by building vessels of greater length than is now the practice from the construction of a vessel under the direction of Earl Stanhope, who is so justly celebrated for his nautical and mechanical knowledge. The sailing of a vessel depends also on the shape of the stem, which (see Fig. 24), if it be upright, and the bow shaped like a wedge, and the angle of incidence between 9° or 10°, it is evident will divide the fluid laterally, part going to the right,

and part to the left; but if the stem be an oblique plane, the water, striking at the same angle of 9° or 10°, is forced downwards under the vessel's bottom, instead of being divided, and offers 57 resistance, whereas the upright stem offers 62. If a vessel under sail always remain in a vertical position, the obliquity of the stern post would be disadvantageous to the steering; but when a vessel inclines, it is of moment; and the obliquity should be greater in small vessels, because they incline more when under sail than larger ships; for suppose a vessel to incline so much as to have the deck perpendicular to the water (or, to use a technical expression, on its beam ends), the rudder, in that case, would have no other effect than to press down the head, and lift up the stern; but if the stern post was inclined aft, then the rudder would have power to turn the vessel. The good or bad qualities of a vessel depend greatly on the shape of those parts which are immersed in the water, by the healing or inclining of the vessel. Now it is evident that the upper part of a vessel may be upright, or parallel to the plane of the masts, or it may project outwards, or incline inwards. A combination of all these is the most advantageous for large vessels, and it should be observed that in all ships, especially in large ones, a certain proportion of a straight line near the surface of the water, termed straight of breadth, is necessary to prevent the vessel from rolling deep; for if a constructor, with a view of making a ship of large dimensions stiff under sail, should make the extreme breadth much above the surface of the water, a wave by striking it to windward would cause it to roll to leeward; and when the sea had passed under the bottom, it would, by acting on the lee side, cause it to oscillate to windward. This double action, by making the ship roll deep, endangers its being dismasted; for every vessel endeavours to accommodate itself to the surface of the water by assuming a perpendicular position with regard to the fluid; for this reason, it is impossible to build and ballast a vessel that shall be easy in all seas. A ship may be considered as a pendulum, making a certain number of vibrations in a given time; and if these vibrations coincide with the set or undulations of the waves, I do not see what can prevent the ship rolling to a great extent, on the same principle that a heavy bell is made by the ringers, with very little force, to swing the whole circle; and navigation would be much more dangerous if the cross waves did not check the rolling of the vessel. But to return to the shape of upper works: should the sides incline inwards, the stability would decrease, as is evident by the experiments with model 19. By the sides projecting, the vessel would be wet, because the gunwhale in bad weather would come sooner in contact with the surface of the water. And another material objection to this shape for vessels which lay in tiers is, that the pressure would be totally confined to the highest part of the top timbers, which would cause them to break off. It may therefore be concluded, that to have a certain part straight (say six feet in a ship of the line) makes the ship easy; that by the sides inclining inwards, the ship is

rendered dry; and by the top sides projecting, more room is given on the upper deck, and less breadth in the channels is requisite for spreading the shrouds which sustain the masts. Vessels 120 feet in length having generally three masts, it would appear that vessels twice that length should have a greater number; and so in all probability they would, but for the idea that the disadvantage of spreading less canvass when going before the wind was the consequence of increasing the number of masts. But suppose a vessel sailing right before the wind seven knots per hour, and by luffing brings the wind two points on the quarter, the vessel now sails on the hypotenuse of a triangle $7\frac{58}{100}$ in length; but if by having more sails draw, the velocity is increased to eight knots per hour, it is evident time is saved, and the objection to a greater number of masts obviated. Then the masts and yards would be lighter, and the diameter of the yards smaller, which would be advantages attending increasing the number of masts. It is a singular fact that by inspecting the logbooks of the East Indiamen 100 years back, their rate of sailing was at least equal to that of the Company's ships at the present period, though copper sheathing is generally introduced. From models of the men of war built in the reign of Charles II. it appears that very little improvement in point of form has taken place since that time. It is not improbable that a greater simplicity in the shape of vessels would prevent the water being thrown into those numerous eddies and whirls which retard the progress of the ship, and injure the steering. The water, when it strikes an opposing body, endeavours to escape by the shortest road, and a globe offers less resistance than any other figure of equal base and altitude.

Sailors and landsmen frequently differ in opinion respecting the stowage. The seaman lays it down as a rule that the stellage, or iron ballast, should be placed as near the centre of the vessel as possible; as the vessel then pitches less, and rolls easier, than it would do if the weight was extended towards the head and stern, and at a distance from the kelson, or middle of the vessel. The landsmen, on the contrary, assert there can be no difference, because the momentum will be equal; for instance, suppose a pair of scales, the beam being of any length, and that a 2 lb. weight be placed in each scale, and the whole made to vibrate, it is evident, provided each arm of the beam be of the same length, the momentum of each must be the same, otherwise the beam would not rest in an horizontal position: consequently the momentum is the same, let the arms be longer or shorter. Suppose the beam 24 inches long, then $12 \times 2 = 24$ and $12 \times 2 = 24$, the momentum on each side. Suppose the beam six inches long, then $3 \times 2 = 6$ and $3 \times 2 = 6$, the same equality as in the first instance; therefore the landsmen maintain it is no matter at what distance the ballast is placed, provided equal quantities are placed at equal distances from the centre of motion. That the practical men are right, and the theoretical men wrong, will appear evident from the

following considerations. Suppose a wave, or any other power, to exert under the bottom of one of the scales a force equal to 2 lb., it is evident that the momentum of the other, or descending scale, would be 24; but in the other case, and under the same circumstances, the momentum would amount to no more than 6, because the length of the descending arm of the ballance was only 3 inches, or a fourth part of the length of the former case. This actually takes place to a certain extent when the head of a ship, after being lifted by a wave, falls into the hollow or trough of the sea; and the same reasoning applies to the rolling; consequently the motion round the longer axis of the vessel will be less when the guns are housed than when run out; and could the whole weight of the vessel be concentrated to a point, the oscillations would be the least possible; but this mode of stowage would inevitably spoil the vessel, by causing it to bend downwards; therefore, to preserve the shape, the lading should be so disposed that the weight put on board each part of the hull exactly equal the weight of the displaced water. Much calculation might be saved, and very useful conclusions drawn, if actual models of ships were submitted to the same process as the models used in these experiments. It would be requisite to find the weight of the displaced water by calculation; and this may be done sufficiently accurately from a draft, as I had a convincing proof, having spent many days in measuring and calculating the capacity of an 80 gun ship while on the stocks. I found the displacement of the water was $3343\frac{2}{10}$ tons of rain water, or $3434\frac{1}{10}$ tons of salt water, the specific gravity of rain water being to that of salt water as 1000 is to 1027. I was favoured with the tonnage of the same ship calculated from the draft, and the difference was only 20 tons, which amounts to rather more than an inch in the draft of water when the ship is loaded, it requiring $18\frac{7}{10}$ tons to bring down the vessel one inch in salt water.

Example.

Model 1. A parallelopipedon. Length, 24 inches. Breadth, 9 inches. Total depth, $7\frac{1}{2}$ inches. Immersion, $4\frac{1}{2}$ inches. Weight of water displaced, 559·06 oz.

TABLE I.

	Exper. 1.	Exper. 2.	Exper. 3.
	Centre of Grav. 2 inches.	Centre of Gr. 2·25 inches.	Centre of Grav. 3 inches.
	Lever, 24·98.	Lever, 24·73.	Lever, 23·98
	oz. d.	oz. d.	oz. d.
5°	2 10 00 3 05 00	2 07 5 2 07 5	1 01 5 1 15 0
	oz.	oz.	oz.
	2 15 00.. 2·9687	2 07 5........ 2·4687	1 00 25........ 1·0156
	Reg. Series...... 2·9146	Reg. Series...... 2·4514	Reg. Series ...... 1·0041
10°	6 04 00 5 11 00	4 15 5 4 15 0	2 02 2 08
	5 15 05.. 5·9687	4 15 25........ 4·9531	2 05 .... 2·3125
	Reg. Series...... 6·0160	Reg. Series ...... 5·0954	Reg. Series ...... 2·2185
15°	9 00 9 10	7 13 5 7 10 0	3 15 5 3 08 0
	9 05 .... 9·3125	7 11 75........ 7·7343	3 11 0........ 3·7343
	Reg. Series...... 9·2783	Reg. Series...... 7·9094	Reg. Series ...... 3·6313
20°	13 03 12 02	10 11 10 12	5 03 5 09
	12 10 5.. 12·6562	10 11 5 ...... 10·7187	5 06 .... 5·3750
	Reg. Series .... 12·673	Reg. Series...... 10·868	Reg. Series ...... 5·2276
25°	16 12 16 00	14 01 13 14	7 07 5 7 02 5
	16 06 .... 16·375	13 15 5 ...... 13·9687	7 05 0........ 7·3125
	Reg. Series ...... 16·188	Reg. Series .... 13·943	Reg. Series ...... 6·9895
30°	19 14 20 12	17 04 17 09	9 03 0 9 15 5
	20 05 .... 20·312	17 05 5........ 17·242	9 09 25........ 9·5781
	Reg. Series...... 19·713	Reg. Series ...... 17·104	Reg. Series ...... 8·8965

TABLE II.

	x	y	x	y	x	y
5°	0·13264	1·5220	0·10920	1·2530	0·04356	0·49983
10	0·26670	1·5359	0·21910	1·2617	0·09919	0·57122
15	0·41610	1·6077	0·34212	1·3218	0·16018	0·61888
20	0·56549	1·6535	0·47416	1·1862	0·23055	0·67409
25	0·73167	1·7313	0·61702	1·4621	0·31366	0·74218
30	0·90758	1·8152	0·76721	1·5344	0·41084	0·82168

TABLE III.

Exper.	5°	10°	15°	20°	25°	30°	Differences.	Corrected Heights.
1	3·5220	3·5359	3·6077	3·6535	3·7313	3·8152	0·0313	3·4942
2	3·5030	3·5117	3·5718	3·6362	3·7121	3·7844	0·0599	3·5480
3	3·4998	3·5712	3·6189	3·6741	3·7422	3·8217	0·0584	3·6018
							0·0706	3·6556
Mean.	3·5083	3·5396	3·5995	3·6579	3·7285	3·8071	0·0786	3·7094
						Mean	0·0538	3·7632

TABLE IV.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Height of Centre of Gravity, 2 inches.						
1·4942	1·5480	1·6018	1·6556	1·7094	1·7632	Value of y.
0·13023	0·26881	0·4146	0·56625	0·72243	0·8816	Value of x.
72·806	150·28	231·77	816·57	403·88	492·87	Momentum.
2·9146	6·0160	9·2783	12·673	16·168	19·731	Regular Series.
Exper. 2.—Height of Centre of Gravity, 2·25 inches.						
1·2442	1·2980	1·3518	1·4056	1·4594	1·5132	Value of y.
0·10844	0·2254	0·3499	0·48074	0·61677	0·7566	Value of x.
60·624	126·01	195·60	268·76	344·81	422·98	Momentum.
2·4514	5·0954	7·9094	10·868	13·943	17·104	Regular Series.
Exper. 3.—Height of Centre of Gravity, 3 inches.						
0·4942	0·5480	0·6018	0·6556	0·7094	0·7632	Value of y.
0·0494	0·09516	0·15576	0·22423	0·29980	0·3816	Value of x.
24·080	53·200	87·078	125·36	167·61	213·34	Momentum.
1·0041	2·2185	3·6313	5·2276	6·9895	8·8965	Regular Series.
3·4942	3·5480	3·6018	3·6556	3·7094	3·7632	Metacentre by exper.

Metacentre by M. Bouguer's theorem, 3·7500.

Experiments with ellipsis. (See model 2.) Length, 24 inches. Breadth, 9 inches. Total depth, $7\frac{1}{2}$ inches. Immersion, $4\frac{1}{2}$ inches. Weight of water displaced, 439·09 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Height of Centre of Gravity, 2 inches above the bottom.						
3·203	3·250	3·298	3·345	3·393	3·440	Metacentre in each exp.
46·03	95·34	147·50	202·04	258·46	316·21	Momentum.
Exper. 2.—Centre of Gravity, 2·25 inches.						
36·41	76·28	119·09	164·49	212·07	261·32	Momentum.
Exper. 3.—Centre of Gravity, 3 inches.						
7·76	19·09	33·85	51·86	72·89	96·67	Momentum.

Experiments with rhombus. (See model 3.) Length, 24 inches. Breadth, 9 inches. Total depth, 7½ inches. Immersion, 4½ inches. Weight of water displaced, 282·66 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 2 inches.						
2·872	2·893	2·913	2·934	2·955	2·976	Metacentre.
21·48	43·81	66·82	90·31	114·08	137·92	Momentum.
Exper. 2.—Centre of Gravity, 2·25 inches.						
15·07	31·42	48·92	67·40	86·69	106·10	Momentum.

Experiments with parallelopipedon. (See model 4.) Length, 24 inches. Breadth, 4½ inches. Total depth, 7½ inches. Immersion, 4½ inches. Weight of water displaced, 279·53 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 1·45 inch.						
2·474	2·486	2·498	2·509	2·521	2·533	Metacentre.
24·96	50·30	75·81	101·30	126·55	151·36	Momentum.
Exper. 2.—Centre of Gravity, 2·25 inches.						
5·47	11·46	17·93	24·81	32·04	39·53	Momentum.
Metacentre by M. Bouguer's theorem, 2·625 inches.						
1·939	1·926	1·911	1·899	1·885	1·872	Centre of gravity when model 4 had one-eighth the stability of model 1.

Experiments with rhombus. (See model 5.) The middle of the immersed part of the model is a right angle triangle, which terminates at each extremity in a right line. Length, 24 inches. Breadth, 9 inches. Total depth, 7½ inches. Immersion, 4½ inches. Weight of water displaced, 140·56 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 2·25 inches.						
4·174	4·174	4·174	4·174	4·174	4·174	Metacentre.
23·57	46·96	69·99	92·49	114·29	135·22	Momentum.
Exper. 2.—Centre of Gravity, 3 inches.						
14·38	28·65	42·71	56·43	69·73	82·50	Momentum.
Exper. 3.—Centre of Gravity, 3·50 inches.						
8·26	16·45	24·52	32·40	40·03	47·36	Momentum.

Experiments with rhombus. (See model 6.) The immersed part of the model is an inverted pyramid. Length, 24 inches. Breadth, 9 inches. Total depth, 7½ inches. Immersion, 4½ inches. Weight of water displaced, 93·75 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
			Exper. 1.—Centre of Gravity, 3·75 inches.			
5·535	5·448	5·360	5·273	5·186	5·099	Metacentre.
17·65	33·74	48·17	60·86	71·74	80·79	Momentum.
			Exper. 2.—Centre of Gravity 4·50 inches.			
8·45	15·42	20·87	24·79	27·17	28·06	Momentum.

Experiments with parallelopipedon. (See model 7.) Length, 12 inches. Breadth, 9 inches. Total depth, $7\frac{1}{2}$ inches. Immersion, $4\frac{1}{2}$ inches. Weight of water displaced, 279·53 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
			Exper. 1.—Centre of Gravity, 2 inches.			
3·503	3·562	3·622	3·682	3·741	3·801	Metacentre.
36·54	75·69	117·12	160·47	205·32	251·24	Momentum.
			Exper. 2.—Centre of Gravity, 2·25 inches.			
30·45	63·55	99·04	136·57	175·78	216·30	Momentum.
			Exper. 3.—Centre of Gravity, 3 inches.			
12·18	27·15	44·78	64·86	87·18	111·48	Momentum.
			Metacentre by M. Bouguer's theorem, 3·750 inches.			

Experiments with parallelopipedon. (See model 8.) Length, 12 inches. Breadth, 10 inches. Total depth, $7\frac{1}{2}$ inches. Immersion, $4\frac{1}{2}$ inches. Weight of water displaced, 312·50 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
			Exper. 1.—Centre of Gravity, 2 inches.			
3·952	3·953	3·978	4·026	4·098	4·194	Metacentre.
53·30	106·00	159·98	216·58	277·15	342·89	Momentum.
			Exper. 2.—Centre of Gravity, 2·25 inches.			
46·37	92·43	139·76	189·85	244·13	303·83	Momentum.
			Exper. 3.—Centre of Gravity, 3 inches.			
23·98	51·73	79·09	109·70	145·08	186·64	Momentum.
			Metacentre by M. Bouguer's theorem, 4·108.			

Experiments with parallelopipedon. (See model 9.) Length, 12 inches. Breadth, 11 inches. Total depth, $7\frac{1}{2}$ inches. Immersion, $4\frac{1}{2}$ inches. Weight of water displaced, 343·75 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 2 inches.						
4·392	4·422	4·461	4·524	4·627	4·789	Metacentre.
71·69	144·59	218·92	296·70	381·67	479·43	Momentum.
Exper. 2.—Centre of Gravity, 2·25 inches.						
64·19	129·66	196·68	267·31	345·35	436·46	Momentum.
Exper. 3.—Centre of Gravity, 3 inches.						
41·72	84·89	129·95	179·13	236·40	307·55	Momentum.
Metacentre by M. Bouguer's theorem, 4·491.						

Experiments with semi-cylinder. (See model 10.) Length, 12 inches. Breadth, 9 inches. Total depth, $7\frac{1}{2}$ inches. Immersion, $4\frac{1}{2}$ inches. Weight of water displaced, 221·05 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 2·25 inches.						
4·406	4·406	4·406	4·405	4·406	4·406	Metacentre.
41·53	82·76	123·35	163·00	201·41	238·29	Momentum.
Exper. 2.—Centre of Gravity, 2·54 inches.						
35·95	71·63	106·76	141·08	174·32	206·24	Momentum.
Exper. 3.—Centre of Gravity, 3 inches.						
27·09	53·97	80·44	106·30	131·35	155·40	Momentum.
Metacentre by M. Bouguer's theorem, 4·50.						

Experiments with elliptical prism. (See model 11.) Length, 12 inches. Breadth, 10 inches. Total depth, $7\frac{1}{4}$ inches. Immrsion, $4\frac{1}{4}$ inches. Weight of water displaced, 245·45 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 2·25 inches.						
4·707	4·735	4·764	4·793	4·821	4·850	Metacentre.
52·56	105·94	159·72	213·47	266·73	319·09	Momentum.
Exper. 2.—Centre of Gravity, 3 inches.						
36·52	73·97	112·08	150·51	188·95	227·05	Momentum.
Exper. 3.—Centre of Gravity, 4·50 inches.						
4·427	10·04	16·78	24·57	33·33	42·95	Momentum.
Metacentre by M. Bouguer's theorem, 4·896.						

Experiments with elliptical prism. (See model 12.) Length, 12 inches. Breadth, 11 inches. Total depth, $7\frac{1}{2}$ inches. Immersion, $4\frac{1}{2}$ inches. Weight of water displaced, 269·85 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 2·25 inches.						
5·153	5·185	5·217	5·248	5·280	5·312	Metacentre.
68·28	137·53	207·21	276·74	345·56	413·11	Momentum.
Exper. 2.—Centre of Gravity 3 inches.						
56·65	102·39	154·83	207·52	260·05	311·39	Momentum.
Exper. 3.—Centre of Gravity, 4·50 inches.						
15·37	32·10	50·07	69·07	89·02	109·54	Momentum.
Metacentre by M. Bouguer's theorem, 5·333 inches.						

Experiments with elliptical prism. (See model 13.) Length, 12 inches. Breadth, 9 inches. Total depth, $6\frac{1}{2}$ inches. Immersion, $3\frac{1}{2}$ inches. Weight of water displaced, 171·80 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 2·25 inches.						
4·272	4·287	4·302	4·316	4·331	4·346	Metacentre.
30·28	60·77	91·23	121·41	151·09	180·02	Momentum.
Exper. 2.—Centre of Gravity, 3 inches.						
19·05	38·39	57·87	77·34	96·64	115·60	Momentum.
Exper. 3.—Centre of Gravity, 3·50 inches.						
11·56	23·47	35·64	47·96	60·34	72·65	Momentum.
Metacentre by M. Bouguer's theorem, 4·357 inches.						

Experiments with elliptical prism. (See model 14.) Length, 12 inches. Breadth, 9 inches. Total depth, $5\frac{1}{2}$ inches. Immersion, $2\frac{1}{2}$ inches. Weight of water displaced, 122·72 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 1·50 inch.						
4·706	4·661	4·616	4·572	4·527	4·482	Metacentre.
39·37	66·41	98·98	128·92	156·98	182·98	Momentum.
Exper. 2.—Centre of Gravity, 2 inches.						
28·94	56·77	83·10	107·94	131·05	152·30	Momentum.
Exper. 3.—Centre of Gravity, 2·50 inches.						
23·59	46·06	67·22	86·95	105·12	121·62	Momentum.
Metacentre by M. Bouguer's theorem, 4·600 inches.						

Experiments with elliptical prism. (See model 15.) Length, 12 inches. Breadth, 9 inches. Total depth, $4\frac{1}{2}$ inches. Immersion, $1\frac{1}{2}$ inch. Weight of water displaced, 73·63 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 1·50 inch.						
6·465	6·127	5·804	5·517	5·266	5·050	Metacentre.
31·99	59·18	82·02	101·16	117·18	130·71	Momentum.
Exper. 2.—Centre of Gravity, 2 inches.						
28·78	52·76	72·49	88·57	101·62	112·30	Momentum.
Exper. 3.—Centre of Gravity, 2·50 inches.						
25·57	46·37	62·96	75·97	86·06	93·89	Momentum.
Metacentre by M. Bouguer's theorem, 6 inches.						

Experiments with right angle triangle prism. (See model 16.) Length, 12 inches. Breadth, 9 inches. Total depth, 7½ inches. Immersion, 4½ inches. Weight of water displaced, 139·77 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 2·25 inches.						
5·669	5·587	5·504	5·421	5·339	5·256	Metacentre.
41·65	80·98	117·71	151·60	182·44	210·07	Momentum.
Exper. 2.—Centre of Gravity, 3 inches.						
32·52	62·78	90·58	115·74	138·14	157·65	Momentum.
Exper. 3.—Centre of Gravity, 4·50 inches.						
14·24	26·37	36·32	44·04	49·54	52·83	Momentum.
Metacentre by M. Bouguer's theorem, 6 inches.						

Experiments with triangular prism. (See model 17.) Length, 12 inches. Breadth, 10 inches. Total depth, 7½ inches. Immersion, 4½ inches. Weight of water displaced, 156·25 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 2·25 inches.						
6·487	6·253	6·070	5·910	5·861	5·835	Metacentre.
57·70	108·60	154·49	197·18	238·48	280·10	Momentum.
Exper. 2.—Centre of Gravity, 3 inches.						
47·49	88·25	124·16	157·11	188·96	221·51	Momentum.
Exper. 3.—Centre of Gravity, 4·50 inches.						
27·06	47·55	63·50	76·94	89·90	104·32	Momentum.

Experiments with triangular prism. (See model 18.) Length, 12 inches. Breadth, 11 inches. Total depth, 7½ inches. Immersion, 4½ inches. Weight of water displaced, 171·88 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 2·25 inches.						
7·116	6·825	6·601	6·445	6·356	6·333	Metacentre.
72·89	136·55	193·58	246·61	298·23	350·94	Momentum.
Exper. 2.—Centre of Gravity, 3 inches.						
61·66	114·17	160·22	202·51	243·75	286·48	Momentum.
Exper. 3.—Centre of Gravity, 4·50 inches.						
39·19	69·40	93·48	114·33	134·79	157·57	Momentum.

Experiments with model 19. The sides incline inwards 45°. Length, 12 inches. Breadth, 9 inches. Total depth, $7\frac{1}{2}$ inches. Immersion, $4\frac{1}{2}$ inches. Weight of water displaced, 139·77 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 2·25 inches.						
5·544	5·240	5·009	4·850	4·765	4·752	Metacentre.
40·13	72·57	99·80	124·31	148·54	174·85	Momentum.
Exper. 2.—Centre of Gravity, 3 inches.						
30·99	54·36	72·66	88·45	101·24	122·43	Momentum.
Exper. 3.—Centre of Gravity, 4·50 inches.						
12·72	17·96	18·40	overset at 16°			Momentum.
Metacentre by M. Bouguer's theorem, 6 inches.						

Experiments with triangular prism. (See model 20.) Length, 12 inches. Breadth, 9 inches. Total depth, 12 inches. Immersion, 9 inches. Weight of water displaced, 279·53 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 5·75 inches.						
7·216	7·216	7·216	7·216	7·216	7·216	Metacentre.
35·72	71·17	106·32	140·18	173·21	204·93	Momentum.
Exper. 2.—Centre of Gravity, 6 inches.						
29·63	59·03	87·99	116·28	143·68	169·98	Momentum.
Exper. 3.—Centre of Gravity, 6·25 inches.						
23·54	46·90	69·90	92·37	114·14	135·04	Momentum.

Experiments with parabolic prism. (See model 21.) Length, 12 inches. Breadth, 9 inches. Total depth, $7\frac{1}{2}$ inches. Immersion, $4\frac{1}{2}$ inches. Weight of water displaced, 186·35 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 2·25 inches.						
4·696	4·696	4·696	4·696	4·696	4·696	Metacentre.
39·73	79·16	117·99	155·92	192·67	227·94	Momentum,
Exper. 2.—Centre of Gravity, 2·70 inches.						
32·42	66·11	96·29	127·24	157·23	186·02	Momentum.
Exper. 3.—Centre of Gravity, 3 inches.						
27·55	54·89	81·82	108·12	133·60	158·06	Momentum.

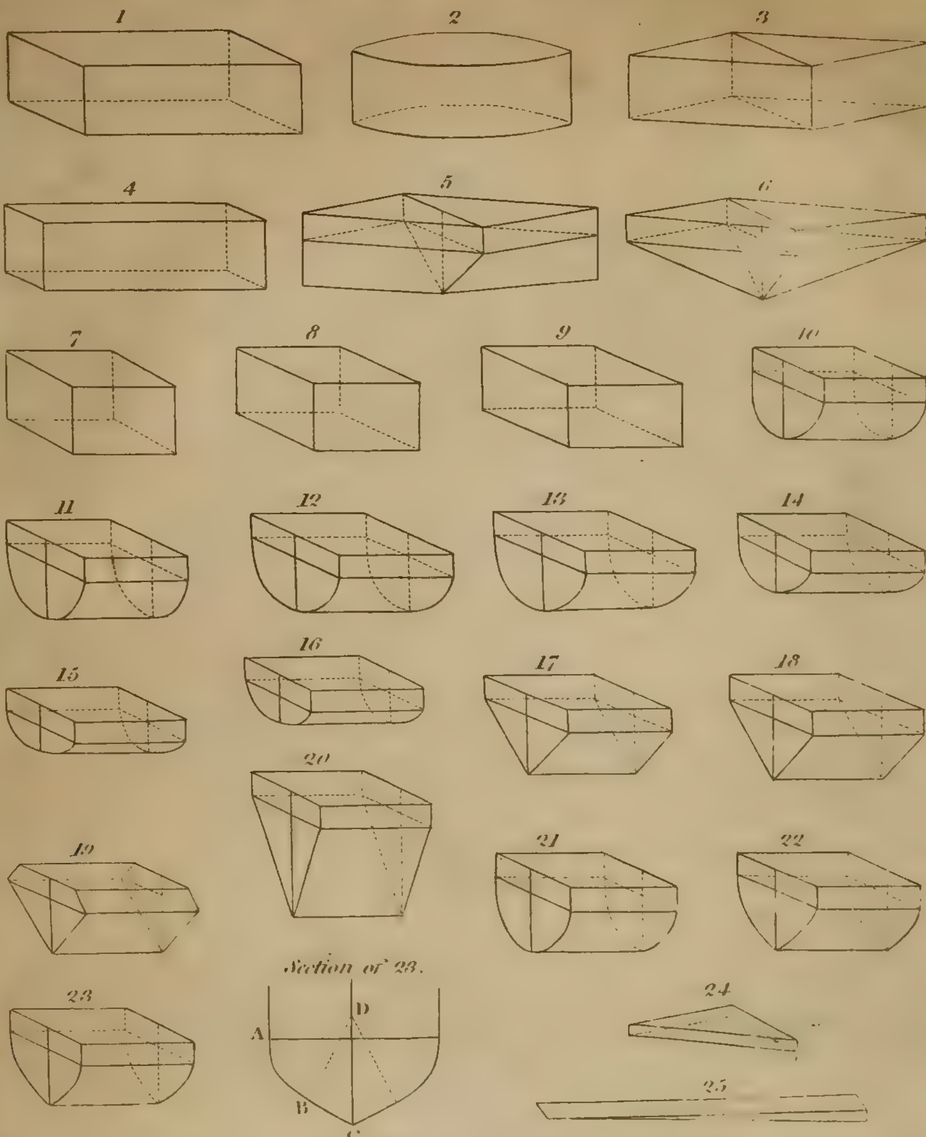
Experiments with prism formed of circular segments. (See model 22.) Length, 12 inches. Breadth, 9 inches. Total depth, 7½ inches. Immersion, 4½ inches. Weight of water displaced, 178·22 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 2·25 inches.						
4·891	4·891	4·891	4·891	4·891	4·891	Metacentre.
41·03	81·74	121·84	161·00	198·94	235·37	Momentum.
Exper. 2.—Centre of Gravity, 2·87 inches.						
31·40	62·56	93·24	123·21	152·24	180·12	Momentum.
Exper. 3.—Centre of Gravity, 3 inches.						
29·38	58·53	87·24	115·28	142·47	168·54	Momentum.

Experiments with compound figure. (See model and section 23.) Length, 12 inches. Breadth, 9 inches. Total depth, 7½ inches. Immersion, 4½ inches. Weight of water displaced, 199·02 oz.

5°	10°	15°	20°	25°	30°	Angles of inclination.
Exper. 1.—Centre of Gravity, 2·025 inches.						
4·873	4·873	4·873	4·873	4·873	4·873	Metacentre.
49·39	98·41	146·68	193·83	239·51	283·37	Momentum.
Exper. 2.—Centre of Gravity, 3 inches.						
32·48	64·72	96·46	127·47	157·50	186·34	Momentum.
Exper. 3.—Centre of Gravity, 4 inches.						
15·14	30·16	44·95	59·40	73·39	86·83	Momentum.
Exper. 4.—Centre of Gravity, 4·50 inches.						
6·46	12·88	19·19	25·36	31·34	37·08	Momentum.

A, B, segment of a circle, of which E is the centre; B, C, being a tangent thereto; the angle, B, D, C, being 26° 34′. (See Plate XLV.)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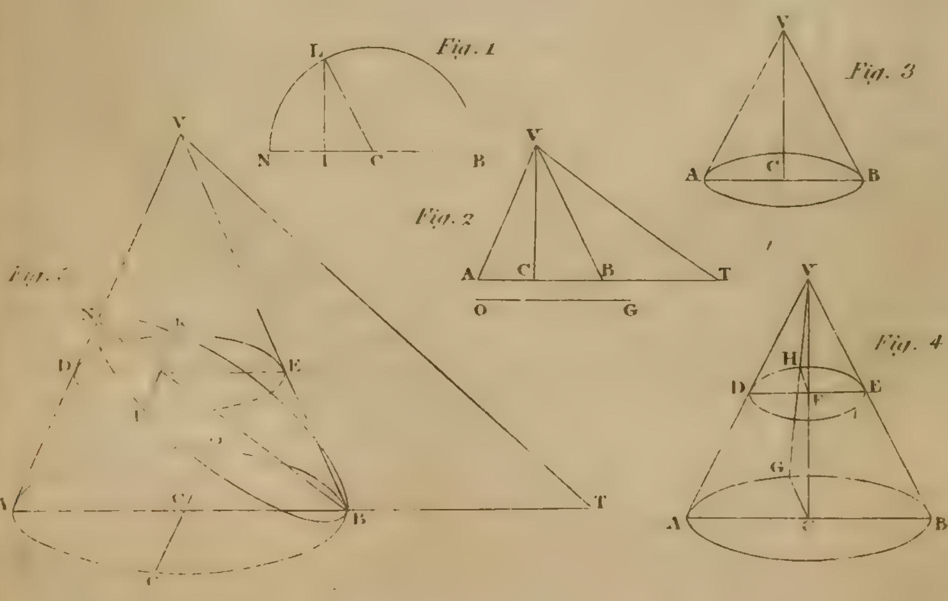
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Article III.

A Demonstration that the Ellipse, when viewed in a certain Position, appears circular. By S.

(To Dr. Thomson.)

SIR, *Jan.* 8, 1816.

It is well known that the circle, if looked upon obliquely, will be projected into an ellipse; but I am not aware that the converse of this proposition has been demonstrated by showing that an ellipse, if viewed in a certain position, will appear circular. This has been established in the following theorems; but it was not the primary object with which they were drawn up. They were occasioned by the wish of getting a scalene cone turned truly in a lathe. Many good workmen assured me that it was impossible; and all the cones of this kind which I have seen (with the exception of that which I am about to mention) have been cut by hand. In thinking on the subject, it appeared that the best way would be to get a cone turned in the first instance with an elliptical base; but here again I met with a difficulty which was not surmounted till within these few months, by a very ingenious friend, who devised the means of executing exactly what I wished. By a mechanical method of trial, he afterwards found where he might cut this elliptical cone obliquely, so that the base should become a circle; but it seemed more satisfactory to investigate the problem mathematically, and I here send you the result. It would be unjust, however, not to add that the first lemma is taken from Dr. Robertson's Conic Sections (8vo. Oxford, 1802), and that several parts of the proposition were suggested by a recollection of the methods used in the first book of that valuable treatise. S.

Lemma 1.—If from any point, I, of a straight line, N B (Plate XLV.* Fig. 1), a perpendicular, I L, be drawn; and if in all cases the rectangle under N I, I B, shall be equal to the square of I L, the curve passing through N L B shall be a semicircle.

For bisect N B in C, and join C L. Then (5, ii.) N I, I B, + $IC^2 = CB^2$; therefore by hypothesis $IL^2 + IC^2 = CB^2$; but (47, i.) $IL^2 + IC^2 = CL^2$; consequently $CB^2 = CL^2$. The same would hold wherever the point I is taken; therefore the locus of the points L must be a curve, which would be generated by the extremity, L, of the given straight line, C L, when its other extremity, C, is fixed, and the straight line revolves about it.

Lemma 2.—Let V A B (Fig. 2) be an isosceles triangle, and O G a straight line longer than the base; then if V T be taken, such that $OG^2 - AB^2 : OG^2 :: VB^2 : VT^2$, we shall have $AB^2 : OG^2 ::$ A T, T B : VT^2. For $AB^2 : OG^2 :: VT^2 - VB^2 : VT^2$; and when V C is drawn perpendicular to (and therefore bisecting) the base, $VT^2 - VB^2 =$ (47, i.) $VC^2 + CT^2 - VC^2 - CB^2 = CT^2 - CB^2 =$ A T, T B (6, ii.).

Definition.—The straight line joining the vertex of the cone and the centre of the ellipse (which forms the base) is the axis of the cone. The only case which bears upon the principal object of the present investigation is when this axis is perpendicular to the base; to this case, therefore, the first, second, and third, propositions are confined; the second, indeed, would be generally true, whatever inclination were given to the axis; but it is not my intention at present to go into the full consideration of the sections of the elliptic cone.

Prop. 1.—Any plane passing through the axis will be perpendicular to the base, and its common section with the conical superficies will be an isosceles triangle.

The first part of the proposition will be true by Eucl. (18, xi.) To prove the second part, let V C (Fig. 3) be the axis of the elliptical cone V A C B, and C the centre of the elliptical base. Then it may be shown exactly, as in the circular cone, that the common sections of the plane and conical superficies will be the straight lines V A, V B; consequently V A B is a triangle. Now as A B is a diameter of the elliptical base, and C is the centre, $AC = CB$. Hence $AC^2 + VC^2 = CB^2 + VC^2$; but by (47, i.) $AC^2 + VC^2 = VA^2$, and $CB^2 + VC^2 = VB^2$; therefore $VA^2 = VB^2$.—Q. E. D.

Prop. 2.—If the conical superficies be cut by a plane parallel to the base, the common section will be an ellipse similar to the base.

Let (in Fig. 4) D H E F be the common intersection of the conical superficies, and of the plane parallel to the base; then D H E is an ellipse similar to the base A G B.

Let a plane pass through the axis, and likewise through any diameter, A B, of the base. Let its common section with the plane D H E be D E, and let the axis meet the plane D H E F in F. Then (16, xi.) D E is parallel to A B, and $DT : AC :: VF : VC :: FE : CB$. Hence $DF : AC :: FE : CB$, and $AC = CB$; therefore $DF = FE$. Now this will be true, whatever may be the position of A B and D E; therefore any line terminated by D H E, and passing through F, will be bisected in that point. Again, let G C be a semidiameter conjugate to A C, and through G C, V C pass a plane, and let its common section with the plane D E H F be H F. Then, as before, H F is parallel to G C, and $DF : AC :: HF : CG$; therefore alternately $DF : HF :: AC : CG$, and (10, xi.) the angle D F H is equal to the angle A C G; and as this will be true of any conjugate diameters, it follows that D H E is an ellipse similar to A G B, and that its centre is at F.

Cor.—$DF^2 : FH^2 :: AC^2 : CG^2$. Hence the rectangle under the abscissæ of D E : the square of its ordinate :: $AC^2 : CG^2$.

Prop. 3.—Let V A B G be a cone with an elliptic base, of which G O is the greater, and A B the smaller, axis; and let the common section of a plane passing through the axis of the cone and the minor axis, A B, of the ellipse, be the isosceles triangle V A B: In A B produced take the point T, such that $OG^2 - AB^2 : OG^2 :: VB^2 : VT^2$. Then by lemma 2, $AB^2 : OG^2 :: AT, TB : VT^2$.

Through B in the triangle V A B draw B N parallel to V T, and through B N pass a plane perpendicular to the plane V A B, and let the common section of this plane with conical superficies be the line N L B K, N L B K shall be a circle.

In B N take any point I, and through I pass a plane parallel to the base. Let the common intersection of this plane, and of the conical superficies, be D K E L, which by Prop. 2 will be an ellipse similar to the base. Let the common intersection of the planes D L E K and N L B K be L K. Now as the axis of the cone is perpendicular to the base, the plane A G B O (18, xi.) is at right angles to the plane V A B, consequently D L K E is at right angles to V A B; but the plane N L B K was made at right angles to V A B; therefore (19, xi.) L K is at right angles to the plane V A B and L I N, L I D are both right angles.

Now as B N is parallel to V T, and D E is parallel to A T, the triangles D I N, A T V, and the triangles I E B, V B T, are similar. Hence D I : N I :: A T : V T, and I E : I B :: T B : V T. Therefore D I, I E : N I, I B :: A T, T B : V T^2; but by construction A T, T B : V T^2 :: A B^2 : O G^2. Therefore D I, I E : N I, I B :: A B^2 : O G^2 :: A C^2 : C G^2.

Lastly, by Prop. 2, D E is the minor axis of D K E L, and by the corollary D I, I E : I L^2 :: A C^2 : C G^2. But it has been shown that D I, I E : N I, I B :: A C^2 : C G^2; therefore D I, I E : I L^2 :: D I, I E : N I, I B; consequently I L^2 = N I, I B, and by lemma 1, N L B is a semicircle.

Cor. 1.—As $\sqrt{OG^2 - AB^2} : OG :: VB : VT, OG : \sqrt{OG^2 - AB^2}$ :: sine of V A B : sine of V T A or N B A. If the cone becomes a cylinder, V A B becomes a right angle, and O G : $\sqrt{OG^2 - AB^2}$ :: radius : sine of N B A.

Cor. 2.—V N L B K will become a scalene cone; therefore any plane parallel to N L B K, or any plane which forms the subcontrary section, will give a circle.

Article IV.

*Letter of Dr. Schübler, Professor of Natural Philosophy and Chemical Agriculture in the Institute, of Hofwyl, to Professor M. A. Pictet, on the Physical Analysis of Soils.**

Hofwyl, July 14, 1815.

In consequence of your desire, during your recent visit to Hofwyl, to know the result of my researches on the physical qualities of arable soils, I do myself the honour of transmitting to you the following comparative table. I shall likewise explain the way in which my experiments were performed:—

* Translated from the Bibliotheque Britannique, Agriculture, p. 248, July, 1815.

	Sp. Gr.	Weight of a French cubic foot and inch. Dry.	Moist.	Property of retaining water. 100 parts of earth retain of water, per cent.	Solidity and consistence of the soil. In a dry state, that of clay being 1000.	In a moist state, adhesion to the surface of a square foot of Iron.	Wood.	Property of drying. Of 1000 parts of water there evaporate at the same time.	Equal quantities require to dry to the same degree.	Absorption of moisture from the air. 1000 parts absorb in a dry state. Hrs.	Gr.	Absorption of oxygen gas from the air. Dry.	Moist.	Specific heat. That of sand being 1000.	Time required by 30 cub. in. of earth to cool from 144° to 70°.	Electricity and Galvanism. Galvanism relative to humus.	Conducting power when dry.
		Cub. f. lbs.	Cub. f. lbs.			With a force of lbs.	lbs.	Parts.	Time.	Hrs.	Gr.		Parts.				
Quartz sand. It occurs in almost every arable soil............	2·75	155·1 c. in. 516 gr.	181·5 c. in. 605 gr.	25	0	5·1	5·7	884	4° 4′	12 24 48	0 0 0	0	2·6	956	3° 27′	—	Non-conductor.
Sand of lime. It often occurs mixed with quartz sand	2·82	158·0 c. in. 506 gr.	188·5 c. in. 628 gr.	29	0	5·5	5·9	759	4 44	12 24 48	2 3 3	0	5·6	1000	3 30	—	Non-conductor.
Potters' clay. A combination of 60 parts clay, and 40 parts of fine sand	2·70	130·4 c. in. 435 gr.	176·5 c. in. 577 gr.	40	373	10·6	11·9	520	6 55	12 24 48	21 26 28	0	9·3	769	2 41	—	Semi-conductor.
Loam clay. A combination of 76 clay with 24 fine sand	2·65	118·0 c. in. 393 gr.	165·5 c. in. 551 gr.	50	488	14·1	15·2	457	7 52	12 24 48	25 30 34	0	11	718	2 30	—	Semi-conductor.
Klayartiger thon. – A combination of 89 per cent. of clay, and 11 of fine sand.........	2·60	107·1 c. in. 357 gr.	159·5 c. in. 531 gr.	61	633	23·0	25·3	349	10 19	12 24 48	30 36 40	0	13·6	684	2 24	—	Semi-conductor.

Clay freed from its fine sand. Composed of 58 silica, 32 alumina, 9 oxide of iron....	2·59	100·3 c. in. 344 gr.	154·5 c. in. 515 gr.	70	1000	36·0	39·0	318	11 17	12 37 24 42 48 48	0	15·3	667	2 19	–	Semi-con-ductor.
Calcareous earth (carbonate of lime). It occurs often with sand of lime	2·46	171·7 c. in. 244 gr.	138·0 c. in. 460 gr.	85	50	19·1	20·8	280	12 51	12 26 24 31 48 35	0	10·8	618	2 10	–	Non-con-ductor.
Humus, or vegetable earth. An essential part of arable land..	1·22	46·4 c. in. 154 gr.	109·0 c. in. 346 gr.	190	87	11·8	12·5	205	17 33	12 80 24 97 48 110	0	20·3	490	1 43	+	Non-con-ductor.
Magnesia (carbonate of magnesia). Rare in arable land .	2·23	21·1 c. in. 75 gr.	101·7 c. in. 339 gr.	456	115	7·8	9·5	108	33 20	12 69 24 76 48 80	0	17·0	380	1 20	–	Non-con-ductor.
Gypsum in fine powder. In a natural state, not calcined ..	2·35	122·6 c. in. 408 gr.	170·2 c. in. 573 gr.	27	73	14·3	15·8	717	5 1	12 1 24 1 48 1	0	2·7	738	2 34	±	Non-con-ductor.
Garden mould, composed of 52 clay, 36·5 quartz sand, 1·8 calcareous sand, 2 lime, and 7·2 humus	2·33	91·7 c. in. 364 gr.	137·0 c. in. 457 gr.	96	76	8·6	10·0	243	14 49	12 35 24 45 48 50	0	18·0	648	2 16	±	Weak semi-con-ductor.
Arable land from a field near Hofwyl, composed of 51·1 clay, 42·7 quartz sand, 0·4 lime sand, 2·3 lime, 3·4 humus.	2·40	112·7 c. in. 376 gr.	158·9 c. in. 529 gr.	52	330	7·8	8·6	320	11 15	12 16 24 22 48 23	0	16·2	701	2 27	±	Weak semi-con-ductor.
Arable land from a valley of Jura, composed of 64 quartz sand, 25·5 clay, 1·2 lime sand, 1·2 lime, 1·2 humus ..	2·52	124·1 c. in. 414 gr.	154·5 c. in. 515 gr.	47	220	7·1	8·0	401	8 58	12 14 24 19 48 20	0	15·0	743	2 36	±	Weak semi-con-ductor.

I ought in the first place to explain why I submit to experiment the earths such as they occur in nature, rather than the pure earths procured by chemical process; though I have not absolutely neglected the latter. I soon perceived that earths prepared artificially differ considerably in their physical properties from those which we meet with in nature. However, it is indispensable, in order to determine precisely the influence of the different earths on vegetation, to acquire an exact knowledge of their secondary principles, (just as is the case in our researches on plants.) The mere knowledge of the primary principles of an arable earth will be of little service. Two different kinds of soil may contain the same proportion of calcareous earth, and yet retain very different proportions of moisture, become dry at very different intervals of time, and consequently produce a very different effect on vegetation, according as the lime is under the form of sand or of a fine powder. Thus 100 parts of calcareous sand only retain 29 per cent. of water, while 100 parts of the same earth in a fine powder retain 85 per cent. This difference is still more sensible in silica. When in the form of sand it retains only 25 per cent. of water; while 100 parts of silica, as it occurs in clay, that is combined with alumina in each arable soil, retains 280 per cent. of water.

Calcareous earth and silica produce under the first form, and when they predominate in an arable field, a hot and dry soil; under the second they render the soil moist and cold. Mere chemical analysis will never be sufficient to point out these striking differences.

These phenomena induced me to chuse for the subjects of my researches the principal species of earth, which form in greater or smaller quantity the upper beds of our globe, of which the arable soil is composed.

I have accordingly examined in the point of view above stated, besides the elements which usually constitute arable soil, namely, silicious sand, calcareous sand, the different kinds of clay, calcareous earth, and vegetable earth, or humus; likewise sulphate of lime and carbonate of magnesia. I paid attention to the latter in consequence of the difference of opinion among philosophers respecting its influence on vegetation. I have likewise subjected some arable compositions to the same experiments by way of instruction and example.

As to the method which I followed in my experiments I send you the following explanation of it.

I ascertained the specific gravities of the earths by hydrostatics. I weighed repeatedly a well corked flask, first filled with water, and then with water and earth at once. By this means I found the comparative weight of the earth to that of water, and consequently its specific gravity. The usual method of finding the weight of a determinate volume of the earth by multiplying its specific weight by that of water, is not applicable in this case, as it is when we ascertain the specific gravity of solid and cohering bodies. It was necessary to weigh determinate volumes of these earths in their

state of perfect dryness, as well as when drenched in moisture; because the weight of the earths varies considerably according to the degree of their humidity. You will find accordingly in the second and third columns of the preceding table, the weight of a cubic foot and inch of each earth examined, given in the medicinal weight of Nuremberg, (the pound containing 12 ounces, *poids de marc*, and the ounce 480 grains.)

Vegetable earth has the smallest specific gravity, and sandy soil has the greatest, whether they be dry or moist. Clay soils, whether dry or moist, are always lighter than sandy soils. I call a soil perfectly dry when, exposed to the temperature of from 100° to 122°, it loses no more weight. I do not venture to expose the soils to a higher temperature, because then the humus would be decomposed or volatilized. I call a soil completely moist when it ceases to drop if put upon a filtering paper. Compound soils are always the lighter the more humus they contain. These researches on the specific gravity of soils lead naturally to the observation that the agricultural terms, *heavy* and *light* lands, are founded on physical qualities quite different from specific gravity.

The fourth column points out the different degrees of force with which the different arable soils retain moisture. I mean by this capacity, the faculty which each soil has of retaining a certain quantity of water, without letting it escape in the form of drops. I usually employ 400 grains of soil, and indicate the quantity of moisture retained at so much per cent.

Of all the substances usually found in arable soil the humus absorbs and retains the greatest quantity of water; almost double its own weight. In this respect magnesia alone surpasses it in a remarkable manner, retaining $4\frac{1}{2}$ times its own weight of water. This remarkable property of pure magnesia must render it prejudicial to vegetation, while it must increase the fertility of a dry and sandy soil. It would appear at first sight that we might calculate the force with which different soils retain water, by comparing a cubic inch of the soil perfectly dry with the same bulk perfectly moist; but as the earths are condensed very unequally when moistened, this comparison would not give an accurate result.

The fifth and sixth columns give the results of experiments on the consistence of the earths, both their solidity when dry and their tenacity when moist. I determined the first of these qualities by measuring their cohesion. For this purpose I formed on a model paralellopipeds of each of the same length, six lines broad and as much in thickness; these I placed upon two supports distant 15 lines from each other. Upon these, when dry, weights were laid successively till they broke. The sum of the weights employed gave me the cohesion. The weight necessary to break the soils containing a great deal of clay, astonished me. No less than 178,300 grains were necessary to break pure clay. I adopted by way of comparison this degree of the cohesion of clay = 1000; the cohesion of sand was = 0.

But when we cultivate a moist earth soaked with water, we have not merely the cohesion to overcome, but likewise the different degrees of adhesion to the plough. I examined this adhesion particularly. I fixed plates of different substances and sizes to the arm of a balance, and determined their adhesion to the different soils by the weight necessary to separate them. To be able to judge more accurately of this adhesive force, I reduced the results of the experiments to what would have taken place if the surface of the adhering plate had been a foot square. The adhesion of clay is the greatest, that of sand the least. It is important to observe that wood adheres always more strongly than polished iron. I repeated the experiment with several sorts of wood, particularly with beech. The cause must be ascribed to the different attractive force of substances, and likewise to their surfaces, which in wood always becomes more uneven when moistened, while that of iron remains the same. This is the reason why oak-wood adheres more strongly to soils than beech. These phenomena explain clearly the meaning of the terms *heavy* and *light* soil, founded entirely on the greater or less difficulty of overcoming the cohesive and adhesive forces. Thus a soil whose cohesion in a state of dryness is only 100, will be easily ploughed; while a soil whose cohesion is 600 will require a more fatiguing exertion.

Columns eight and nine show the results of my comparative experiments on the evaporation of the soils. To find the quantity of water which each kind allows to escape in a given time, and at a given temperature, I spread upon a thin plate of iron an even layer, containing a given quantity of the soil soaked with moisture, and expose it in a close room for four hours to the temperature of 55°. The diminution of weight during that time gives me the quantity of moisture evaporated. For each experiment I employed the same quantity of soil, namely 200 grains, spread upon a surface of 10 square inches.

I found in the same manner the difference of time which the different soils require to become dry to the same degree, or to lose the same quantity of water by evaporation. This I could calculate with great exactness.

The 10th column contains the comparative power of the different soils to absorb moisture from the atmosphere when exposed to it. For these experiments I always employed the soils in a state of complete dryness. I spread equal quantities of each soil upon equal surfaces (200 grains upon 10 square inches) which I placed upon stands under equal sized bell glasses, standing upon water to keep the air always uniformly saturated with moisture. I ascertained the increase of weight in 12, 24, and 48 hours. The absorption was greatest at first, and diminished in proportion as the soils imbibed humidity, and ceased entirely after some days, when they were saturated with it. The absorption of humus surpasses that of all the others, even of magnesia.

Columns 11 and 12 show the proportion of oxygen gas absorbed

by the different soils exposed to the atmosphere. Several years ago Mr. Alexander Humboldt drew the attention of naturalists to this remarkable quality of soils. Afterwards some philosophers denied that it existed. This induced me to make new and exact experiments on the subject. I chose glass globes of the same size, which I could seal hermetically. I put into them equal quantities of the different soils, and left them for 30 days in the temperature of between 61 and 66°, completely shut out of all contact with the atmospheric air. I then examined the air in the globes by means of the eudiometer of Volta. The proportion of oxygen absorbed differed extremely, according to the degree of the dryness or moisture of the soils. When the soils were quite dry no oxygen was absorbed, or at least very little. I then exposed these soils in the month of May for several days to the open air, and shut them up again after they had absorbed some moisture, The air in which they were confined now exhibited evident marks of the absorption of oxygen. I made the experiment a third time, employing soils soaked with moisture. In this state they absorbed a considerable quantity of oxygen, as will appear from the table, while water itself, in the same time, had absorbed only a very minute quantity of oxygen. The excessive quantity absorbed by magnesia surprised me; but reiterated experiments with pure magnesia confirmed it. The oxygen thus absorbed does not appear to combine chemically. Drying the earths and exposing them to a higher temperature, deprived them of the gas absorbed, which they absorbed again when exposed to a new experiment. Humus alone presents an exception. A portion of its carbon combines with the oxygen. Carbonic acid gas is formed and escapes by evaporation.

To remove altogether the objection that the oxygen is absorbed by the water rather than by the earths, I made another series of experiments. I poured water upon the earths till each was covered by two lines of fluid, and proceeding in the manner described above I obtained the same result; humus and clay absorbed a great deal of oxygen gas, sand very little.

Columns 13 and 14 show the different specific heats of the soils. In my experiments on this subject I followed different methods. First I mixed the soil with water raised to different degrees of heat, then I exposed each in Lavoisier's calorimeter; finally, I heated equal quantities of soil to a determinate temperature, and then ascertained the time requisite for each to cool down to another determinate degree. The general results obtained by these different methods were the same. Sand always exhibited the maximum and magnesia the minimum of specific heat, when the volume of each was the same; the only mode of comparison which appears to me just when we speak of great masses of soil. On this basis, fixing the specific heat of the sand of lime at 1000, I found the numbers contained in the 13th column. Of the three methods above mentioned the last appears to me the best, as being the most

proper to conduct us to the object of our research. It is in fact by this method that we acquire a knowledge of the degree of force with which the soil retains heat, a power on which its specific heat and its conducting faculty depend. This method is likewise much easier and more certain than the two first when applied to the soils. It is by the difference in the time during which they retain their heat that great masses of soil are chiefly distinguished in nature.

The two last columns contain the relation of the different soils with respect to electricity and galvanism. When the dry earths are scraped with a knife, and the scrapings allowed to fall upon the plate of an electrometer, they all, even humus, exhibit negative electricity. When perfectly dried they are non-conductors, excepting argillaceous earths, which are semi-conductors, owing to the iron and the humidity of which they are never totally deprived.

With respect to galvanism the humus is distinguished from the other soils in a remarkable manner. The ordinary soils are all on the negative side of the galvanic column; humus alone is on the other side. I employed humus dissolved in different menstrua, namely, water, lime-water, potash, or soda-water, or water impregnated with sulphate of lime. In all these cases the humus was precipitated in brown flocks round the positive pole. This often took place in a few minutes, while the alkalies and earths were collected round the negative pole. I conceive it to be important to attend to these galvanic relations of the earths, before preceeding to their chemical decomposition, as for example, that of humus into carbon and different gases, and of the earths into metals and oxygen. Would it be impossible to form a galvanic pile by alternate arrangements of humus and the other arable soils? I have not hitherto been able to procure a sufficient quantity of humus to try the experiment. The galvanic phenomena above indicated were obtained by means of piles of 45 pair of plates, each an inch in diameter.

These details I conceive will enable you to judge of my experiments. For my own part I am satisfied that the chemical analysis of any fertile soil whatever would not be sufficient to make us acquainted with it in all its relations, and to assign it its true place in agriculture: for the physical properties of soils composed of the same chemical constituents may be very different, according to the different forms and modes in which the simple earths are combined in the different compound soils, from which important phenomena result, of which all the species of soils furnish examples.

Far from considering my researches on phenomena so interesting to vegetation and agriculture as terminated, I have resolved (as far at least as depends upon myself) to continue them, without neglecting any thing which may lead to more general and certain results. At present I am employed in further experiments on the absorption of oxygen by the earths; on the different degrees of heat which they acquire from light; and on their different influence in a

state of purity on the growth of plants. I propose to communicate to the public in the fifth number of the Feuilles Economiques of Hofwyl, the subsequent details of all these experiments, with their particular relation to rural economy.

G. Schübler.

Article V.

Vindication of Mr. Dalton's Theory of the Absorption of Gases by Water, against the Conclusions of Saussure. By Mr. John Dalton.

(To Dr. Thomson.)

RESPECTED FRIEND, *Manchester, Jan.* 22, 1816.

In the *Annals* for November I find a paper by M. de Saussure on the absorption of the gases by liquids. I read it with some interest, as a subject which engaged my attention pretty fully some years ago, and which indeed I have never since lost sight of. It gave me great surprise that the conclusions I deduced from my experiments should have been so far misunderstood by so acute and able a writer as Saussure, and I flattered myself that his readers could scarcely fail of seeing that his animadversions on what he calls my theory of the absorption of gases are in great part misapplied; unless it be assumed as an axiom, that if from equal measures unequal measures be taken there will remain equal measures. But when I find you have translated the Essay without discovering the misapplication, and two months afterwards reviewed the same, and still seemed to remain in ignorance, I fear that my deductions must have been stated in such way as not to be easily understood, at least by the generality of readers who may be said to run and read.

When water deprived of all air is agitated along with a given volume of any gas or mixture of gases, and at the same time subject to a given pressure as that of the atmosphere, after a few minutes an equilibrium takes place, or the water having imbibed a certain quantity ceases to imbibe more of the gas. Now my hypothesis is, that certain uniform relations exist in regard to the density of the gases whether simple or mixed, in and out of the waters, *after the process of absorption has ceased*; but it says nothing at all as to the relations out of the water, *prior to the absorption.* In some circumstances the cases are indeed in effect the same, and no distinction is of course necessary; as for instance, when water is impregnated with atmospheric air in communication with an unbounded volume of the air, or when the several gases subjected to the absorption are equally absorbable; but in instances such as Saussure has given, where the volumes of gases are limited and unequally absorbable, the two cases are as widely different as

any one chuses to make them. No doubt an expert analyst might upon my principles find formulas for the residuary gases, by having given the total volume of each individual gas in any mixture, and the rates of their separate absorptions by water; but I can assure M. Saussure, as well as yourself, that it is to me no very easy task; I shall be thankful to receive them from either of your hands; whereas the other problem, to find formulæ for the total volumes of the gases, from the residuary gases and the rates of absorption being given, is very easy; they are as under:—

Let a, b, c, &c. be the residuary volumes of the different gases A, B, C, &c. after the absorption; w, the volume of water; $\frac{w}{m}$, $\frac{w}{n}$, $\frac{w}{p}$, &c. the rates or portions of the gases A, B, C, &c. in an unmixed state respectively absorbed by water, ascertained by previous experiments. Then the formulæ representing the total quantities of the respective gases in and out of the water together will be, according to the principles which I maintain, as follows:—

$$\text{Original volume of A} = a + \frac{w}{m} \cdot \frac{a}{a + b + c, \&c.}$$

$$\text{Ditto,} \quad B = b + \frac{w}{n} \cdot \frac{b}{a + b + c, \&c.}$$

$$\text{Ditto,} \quad C = c + \frac{w}{p} \cdot \frac{c}{a + b + c, \&c.}$$

and the total volume of all the gases in and out will be $a + b + c$, &c. $+ \frac{w}{a + b + c, \&c.} \cdot \overline{\frac{a}{m} + \frac{b}{n} + \frac{c}{p}} + \&c.$

These formulæ comprise the essentials of the theory as first announced by Dr. Henry (see Nich. Jour. v. 240, 1803), and I will undertake to prove in what follows that Saussure in all his numerous experiments has not given one result that militates against it. It is true I have further maintained that the formulæ may be restricted a little; that is, that the values of m, n, p, &c. are limited to the cubes of the natural series 1, 2, 3, 4, &c.; and in conformity with this limitation have suggested a principle of equilibrium which pretty obviously arises out of it; but if the values of m, n, p, &c. in certain cases are not those I have stated, then it follows that in such cases the equilibrium can be adjusted without any especial regard being had to the distances of the particles of air within the liquid being multiples of those without.

Let us now compare the results of the above formulæ with all those of Saussure ascertained by experiment, and of which he asserts that "none of them correspond with Dalton's theory.

1. *Mixture of Carbonic Acid and Hydrogen.*—According to Saussure, water takes 1·06 times its volume of carbonic acid, and $\frac{46}{1000} = \frac{1}{22}$ of its volume of hydrogen. By experiment, he ascertained that in a certain instance 100 measures of water being impregnated with a mixture of these gases, the residues were—carbonic acid, 173; hydrogen, 213·5; query the total volume of the

two mixed gases before the absorption? Here we have $a = 173$, $b = 213{\cdot}5$, $m = \frac{1}{1{\cdot}06}$, $n = 22$, and $w = 100$. Hence

$$\begin{array}{lr} & \text{Measures.} \\ \text{The orig. vol. of carb. acid} = 173 + \frac{100 \times 1{\cdot}06}{1} \cdot \frac{173}{386{\cdot}5} = & 220{\cdot}5 \\ \text{and that of hydrogen} = 213{\cdot}5 + \frac{100}{22} \cdot \frac{213{\cdot}5}{386{\cdot}5} = & 216 \\ \text{Total} & 436{\cdot}5 \end{array}$$

The actual volume of the mixture by experiment was 434. Hence in this instance the theoretic result *exceeds* the experimental one only by $2\frac{1}{2}$.

2. *Mixture of Carbonic Acid and Oxygen.*—Saussure finds water to absorb $\frac{65}{1000} = \frac{1}{15}$ or $\frac{1}{16}$ of oxygen. 100 measures of water saturated with a mixture of carbonic acid and oxygen left 147·9 carbonic acid and 190 oxygen. Here $a = 147{\cdot}9$, $b = 190$, $m = \frac{1}{1{\cdot}06}$, $n = 16$, and $w = 100$. Hence

$$\begin{array}{lr} & \text{Measures.} \\ \text{The orig. vol. of carb. acid} = 147{\cdot}9 + \frac{100 \times 1{\cdot}06}{1} \cdot \frac{147{\cdot}9}{337{\cdot}9} = & 194{\cdot}3 \\ \text{and that of oxygen} = 190 + \frac{100}{16} \cdot \frac{190}{337{\cdot}9} = & 193{\cdot}5 \\ \text{Total} & 387{\cdot}8 \end{array}$$

The actual volume of the mixture by experiment was 390: hence in this instance the theoretic result *falls short* of the experimental one only by 2·2.

3. *Mixture of Carbonic Acid and Azotic Gas.*—Water was found to take $\frac{41}{1000} = \frac{1}{24}$ of azotic gas; and 100 measures saturated with a mixture of carbonic acid and azotic gas, left 134·9 carbonic acid and 175·5 azote. Here $a = 134{\cdot}9$, $b = 175{\cdot}5$, $m = \frac{1}{1{\cdot}06}$, $n = 24$, and $w = 100$. Hence

$$\begin{array}{lr} & \text{Measures.} \\ \text{The orig. vol. of carb. acid} = 134{\cdot}9 + \frac{100 \times 1{\cdot}06}{1} \cdot \frac{134{\cdot}9}{310{\cdot}4} = & 180{\cdot}9 \\ \text{and that of azotic gas} = 175{\cdot}5 + \frac{100}{24} \cdot \frac{175{\cdot}5}{310{\cdot}4} = & 177{\cdot}9 \\ \text{Total} & 358{\cdot}8 \end{array}$$

The actual volume of the mixture by experiment was 357·6. Hence in this instance the theoretic result *exceeds* the experimental one only by 1·2.

4. *Mixture of Azote and Oxygen.*—In water saturated with atmospheric air we have $a = 79$, $b = 21$, $m = 24$, $n = 16$, and $w = 100$. Hence

	Measures.
The original volume of azotic gas $= 79 + \frac{100}{24} \cdot \frac{79}{100} =$	82·39
Ditto oxygen - - - $= 21 + \frac{100}{16} \cdot \frac{21}{100} =$	22·31
	104·60

The actual volume of the mixture absorbed by experiment, according to Saussure, was about five per cent.; it appears by theory to be 4·6; hence the theoretic result *falls short* of the experimental one by a little.

These four experiments are all that are given on the absorption of mixed gases; the results are all as nearly accordant with theory as we have a right to expect, two deviating a little on one hand and two on the other. It appears then that instead of "none of these corresponding with the theory," they all corroborate it in as striking a manner as if they had been selected out of a large number expressly for the purpose of supporting the theory.

It may be expected from the above remarks that I consider the results of the above four experiments as nearly approximating to the truth. It is not so. The absorption of atmospheric air, as well as oxygen and azote, I am convinced, is not much more than *half* what Saussure states; but his errors in azote and oxygen being nearly proportional, the comparative quantities in the mixture are not much affected.

In fact Saussure is wrong in all the less absorbable gases; his method with them is radically bad, that of taking much air and little water, and endeavouring to find the air absorbed by working the whole volume of air previous to the absorption and the reduced volume. It may be specious enough upon paper, but it is not practically the most accurate way to obtain one grain of any article by first weighing 1000 grains, and then weighing 999 grains from that mass. I apprehend Saussure ascertained the capacity of the flask M when dry within: but when filled with air for the experiment it was wet within, and consequently did not contain so much air by the quantity of water adhering to the glass; this circumstance, trifling as it may seem, is nearly sufficient to account for the differences betwixt his results and mine.

In making the above assertion I do not mean to maintain that the results which Dr. Henry and I obtained are free from inaccuracies; I believe Saussure in one or more instances has corrected us in regard to the absorption by waters; as for other liquids it is more particularly a case which concerns Saussure and myself, on which I shall remark presently; but I do maintain, that the results published by Dr. Henry and me are incomparably nearer the truth than Saussure's, particularly with the less absorbable gases. It appears to me a great pity that he should have published his results without subjecting them to certain checks which could not fail to present themselves to his observation. For instance, it would have been desirable to find that five per cent. of atmospheric air is

obtained from water by heat, by the air-pump, &c. to corroborate the absorption. The best and readiest method of all is to expel the atmospheric air by hydrogen, in which case the air expelled and the hydrogen absorbed are immediately determined by well known methods; but to those who do not understand the theory of this process it can be of little use. The quantity of oxygen in water is accurately ascertained by agitating with nitrous gas, as I stated in the paper 12 years ago, allowing 3·4 nitrous for 1 oxygen, or perhaps more correctly 3·6 for 1. The point of saturation is when neither of the gases is found in the residue. Another very easy method of determining the oxygen in water has recently occurred to me, and in about a dozen trials I have found it never deviate, though made upon very different quantities of water, namely from five ounces to 200; it has one advantage too, that it requires no great skill either to execute or comprehend it. I will relate one of the experiments.

18 ounces of clear rain-water and three of lime-water were briskly agitated together in a tall cylindric jar, so as to acquire a full charge of atmospheric air; after standing a few minutes 13 grain measures of a solution of green sulphate of iron (1·157 = $3\frac{3}{4}$ salt = 1 oxide) were put into the water and gently agitated with a rod for five minutes. In half an hour a pure *yellow* oxide had subsided. The water was drawn off by a syphon, and seven grains more of the sulphate were put in and agitated as before; in a quarter of an hour a perfectly *green* oxide had subsided, which preserved its colour for many days under the water. Now if Saussure's estimate of oxygen in water be admitted, the last should have been a perfect *yellow* oxide. Allowing $\frac{1}{9}$ of the weight of the green oxide for the additional oxygen, we find about 80 grain measures of oxygen gas in 21 oz. of water, or 10080 grains. If the water had been charged with pure oxygen the quantity would have been 400 grains, or nearly four per cent., instead of 6·5, and five times the quantity of sulphate would have been required, as I have repeatedly found. No oxygen is found in water after the green oxide begins to be permanent.

Though the quantity of oxygen gas absorbed by water appears to me decisively to be 3·7 per cent. as I before determined, or perhaps 4 ±, when recent agitation in the purest gas has been used, yet I do not find the same degree of accuracy in my former results in regard to azote; I have repeated some of my experiments and find that water takes $2\frac{1}{2}$ per cent. of azote as nearly as possible, and not 1·56 as I formerly stated, nor yet 4·1 as Saussure states. Both Henry and Saussure are wrong I believe in placing azote below hydrogen; this last is the least absorbable; water takes very nearly two per cent. of hydrogen. Henry says, 1·6, and Saussure, 4·6; but it must be understood that both of these chemists profess to give the observed absorption by water *purified by boiling* only, and not absolutely pure; whereas the numbers I have given above are understood of absolutely pure water. If we allow that $\frac{1}{6}$ part of

the air still remains in water after long boiling (which is no uncommon circumstance); then Henry's numbers would be 1·9 for hydrogen, and Saussure's 5·5, making an enormous disproportion in their errors.

When water is violently agitated with any kind of gas, it is some time before the surplus gas mechanically diffused through the water in small bubbles makes its escape: after all there is probably a surcharge; the quantity of the surcharge I have endeavoured to investigate; it is an object of some consequence in a theoretic point of view. The following may be of some use to such as choose to take up this subject. When a gallon of water (or about $\frac{1}{6}$ of a cubic foot) is well boiled, and then poured into a cylindric jar, so as to be about eight inches deep, and suffered to remain exposed to the atmosphere without agitation, it recovers one half of its air in two days. In 10 days no further absorption is remarked; but by violent agitation it will take $\frac{1}{10}$ or $\frac{1}{12}$ of a full charge. It should be known that water requires about one minute's agitation to acquire a full charge of any gas, when previously as free as possible from air, supposing the water to be about 20 times the volume of air; but if the water be 30 or 40 times the air, it may require two or three minutes' agitation. I seldom allow less than five minutes.

It is not very obvious to me why Saussure has passed over an important feature of the theory of absorption without any notice I can find. I mean the effect of heat. He has some allusions to it in the paragraph announcing his modes of freeing liquids from air; but they are evidently accidental, and he enters upon no explanation. He suggests two methods of expelling air (or rather suffering it to escape) from liquids; the one is, by removing the incumbent air from their surfaces and substituting steam of equal pressure (that is, boiling the liquids); the other is, by removing the incumbent air and substituting steam of very weak pressure, (that is, by the air-pump.) He observes that in the last case "the pressure of the vapour prevents the escape of the air." It would be very reasonable to ask why the pressure in the former case does not much more prevent the escape of the air. The only answer, I conjecture, that Saussure and you could give, would be that the heat of boiling water expels the air with so much force, that a counteracting pressure equal to that of the atmosphere is insignificant. The truth is that neither the heat nor the vapour has directly any influence upon the expulsion or retention of the air. If a tube half filled with water and half with air be hermetically sealed, it may be put into freezing water, or boiling water, which ever we please, for half an hour, and no air will be observed to pass either into or out of the water, though the pressure of the incumbent steam varies from 1 to 150, and the temperature from 32° to 212°. The same tube open at the end immersed in boiling water would in half a minute be quite opake with ascending air bubbles.

M. Saussure and you have represented it as my theory, that "all liquids" absorb the same quantity of air as water does. (p. 339,

and vol. vii. p. 23.) My words are "most liquids —— except." Those expressions, are, I believe, not generally deemed synonymous. My experiments were much more numerous on water than on other liquids, and what I had more particularly in view was to show that any slight modification of water by acids, salts, &c., such as might naturally occur, was not sensibly distinguishable from pure water in regard to absorption. On concentrated liquid acids and saline solutions I had not made many experiments. A strong solution of common salt I found to absorb only one third of the volume of any gas that water did, and this was the reason of my saying "most" instead of "all liquids —— except;" but guarded as the expression was, I am ready to allow that it was not sufficiently so. Saussure's copious experiments on other liquids than water far surpass mine in number and variety, and will be found a valuable acquisition, if the accuracy of the less absorbable gases be equal to that of the more absorbable ones.

If the influence of chemical affinity did not exist, the gases would be absorbed by all liquids in the same order, according to Saussure; and finding them not so, he concludes, that the absorptions are occasioned by affinities. A better distinction in my opinion would be, that if a volume of any gas or mixture of gases is absorbed by water in proportion to the pressure of the incumbent gas, and the same volume is capable of being expelled again unchanged by the usual means of boiling, the air-pump, or agitation with any other gas, then the absorption is mechanical; but if a change in the quantity or quality of the gases expelled be observed, it must be ascribed to affinity: thus when nitrous gas or sulphureted hydrogen are pressed into pure water freed from all air, we can rarely if ever recover the same quantity again, the respective gases being in a short time partially decomposed. If it be said that solutions of ammonia, muriatic acid, &c. in water, must upon these grounds be considered as mechanical combinations; I grant they are combinations of a mixed nature, partly mechanical and partly chemical. The immense condensation of volume of those gases by water cannot be accounted for on mechanical principles alone; the water must have an affinity for the bases of these gases, or for their caloric, or both, and besides the quantity is not as the pressure. But when no condensation of gas takes place, and the quantity is accurately as the pressure, to call this a case of affinity seems to me just as reasonable as to ascribe the air in a sand-hill to the chemical affinity of sand for air, and to argue that that affinity varies according to the state of the barometer.

It may not be amiss to sum up these remarks under a few heads, exhibiting the leading principles of the theory of absorption which I adopt, in order that they may be more clearly understood. The gases are of course chiefly those of which water does not take more than its bulk.

1. The quantity of any pure gas which water absorbs is in proportion to the pressure or density of the gas.

This was Dr. Henry's discovery; but I adopt it as an essential principle of the theory. Saussure also confirms it.

2. The quantities of any mixture of gases which water absorbs are also in proportion to the pressure or densities of the several incumbent gases *after the absorption has ceased*, (but not in proportion to their pressures before the absorption, unless these two ratios happen to be the same); and are the same as if the gases were alone, allowing for the diminished density.—Thus, water charged with atmospheric air of unlimited volume contains $\frac{21}{100}$ of a full charge of oxygen, and $\frac{79}{100}$ of a full charge of azote; but if water be charged with a limited volume of air, as $\frac{1}{15}$, then it will contain less oxygen and more azote than specified above.

This was a discovery of mine; it is confirmed by the experiments of Henry, and by the four experiments of Saussure above explained, the only ones of his that apply to it.

3. Heat and cold, or change of temperature, has no influence on the quantities of gas absorbed by water.

This was an observation of mine; the idea was at first suggested by a consideration of other facts, and afterwards confirmed by experiment. As heat increases the force of the incumbent air in proportion as it increases that of the air in the water, the equilibrium is not disturbed. The reason why heat seems to expel air from liquids is that it generates steam, which removes the atmospheric air from the surface. The air-pump, or hydrogen gas, will remove the pressure of the azote and oxygen of the atmosphere, and are equally efficacious in expelling the air from water without heat.

This feature of the theory, as has been observed, has not been noticed by Saussure.

4. The quantities of the several gases absorbed by water are as 1, $\frac{1}{8}$, $\frac{1}{27}$, $\frac{1}{64}$, &c.; the volume of water being unity.

This observation occurred to me during the investigation, and I attempted to show that these proportions necessarsly resulted from the preceding phenomena. Though many of Saussure's results differ widely from the above proportions, in consequence of their being erroneous as shewn above, yet it must be allowed that some exceptions occur in regard to this law, particularly in the class which should be $\frac{1}{64}$ absorbable: whether the approximations are accidental, or whether they are founded on the principle of equilibrium I have suggested, may be a fair subject for future discussion when the facts are ascertained beyond doubt.

The assertion that I made, that "most liquids freed from viscidity, such as acids, alcohol, liquid sulphurets, and saline solutions in water, absorb the same quantity of gases as pure water, except they have an affinity for the gas, such as the sulphurets for oxygen, &c." appears to me to be too general and comprehensive. Saussure has clearly shown that oils, acids, alcohol, and saline solutions differ very materially from waters in the quantity of gases absorbed; but for any thing that appears these liquids all agree with water in the other three primary laws.

I must now leave it to be determined whether "it would appear from these experiments of De Saussure, that Mr. Dalton's theory [of the absorption of gases by liquids] is erroneous in every particular." I remain respectfully yours,

JOHN DALTON.

ARTICLE VI.

Defence of the Objections to Prevost's Theory of Radiant Heat. By John Murray, M. D. F.R.S. E., Lecturer on Chemistry in Edinburgh.

(To Dr. Thomson.)

SIR, *Edinburgh, Jan.* 20, 1816.

IN the general view you have given in your last Number of the late improvements in science, you mention that Mr. Davenport had written a very complete refutation of some objections that had been started against Mr. Prevost's theory of Radiant Heat. This refers, I believe, to the answer given by that gentleman to some objections to the application of that theory to radiant cold. One of these objections I had advanced, and I now take the liberty of making a few observations on Mr. Davenport's reply to it, which I was prevented by circumstances from doing at the time it was published.

When a tin cannister containing a freezing mixture is placed opposite to a reflecting metallic mirror, in the focus of which a thermometer is placed, if one of the surfaces of the cannister be covered with a coating of lamp-black, it is known that the depression of temperature which is indicated by the thermometer, is much greater than when the clear metallic surface is opposed. This appears to me inconsistent with Prevost's explanation of radiant cold. That explanation assumes that the effect depends merely on the interchange of rays of heat between the thermometer and the cold surface, regulated by the mirror, the thermometer being at a higher temperature, and therefore giving off more radiant heat than it receives in return from the cold body; so that its temperature falls. Now it seems obvious that of different surfaces giving off different portions of caloric by radiation at the same temperature, the one which gives heat will allow of the greatest depression of temperature in the thermometer, for it is the one which will make the least return. A metallic surface is that which radiates least, it therefore should cause the greatest degree of cold when opposed to the thermometer; but it causes the least, and the blackened surface which discharges the largest quantity of caloric by radiation is the one which, in this experiment, causes the greatest depression of temperature in the thermometer.

Mr. Davenport's reply (which has been considered as satisfactory

by Mr. Prevost) is that the effect depends on the power of the different surfaces in reflecting radiant heat. Of the heat radiated from the thermometer a portion is always returned by reflection. A blackened surface, it is remarked, radiates much, but it reflects little; while it intercepts radiation or reflection from behind. A metallic surface radiates less, but it reflects as much as it fails to radiate; hence it is inferred, that by reflecting so much heat, though it radiates so little, it is powerful in counteracting the fall of the thermometer.

In this reasoning it appears to me that too little effect is ascribed to radiation, and too much to reflection. The comparative powers of different surfaces in producing the phenomenon of radiant heat show how much more influence is due to the radiating than to the reflecting power; the blackened surface which reflects scarcely any producing, when opposed to the thermometer, the greatest heating effect, because it radiates most; while the metallic surface, which returns the largest quantity by reflection to the thermometer, still produces the least heating effect, because it is inferior in radiating power. In the experiment with radiant cold, the same difference of effect ought to take place; the blackened surface, though reflecting little, still by its superior radiating power ought to produce the greatest heating effect, so as to counteract the fall of temperature in the thermometer; and the metallic surface, though it reflects best, yet emitting so little by radiation, ought to produce the least heating effect, and therefore admit of the greatest depression of temperature in the thermometer, all of which is the reverse of the fact. It seems to me, therefore, that the original argument is still just, and that, according to Prevost's hypothesis, the blackened surface ought to be least powerful in producing radiant cold. Or if even the circumstance of its inferior reflecting power should so far counterbalance its superior radiating power as to render it equal to the other, still no cause can be assigned for its cooling agency being so greatly superior. "A blackened surface," says Mr. Davenport, "radiates much it is true, but it intercepts an equal volume of radiation or reflection from behind; a polished surface radiates less, but it reflects as much as it fails to radiate." Its power, therefore, ought to be the same as that of a surface reflecting little; but which *radiates* as much as it fails to reflect; that is, the power of the two surfaces ought to be the same, and there is no cause why the blackened surface should be so far superior to the other in producing cold.

I am Sir, yours respectfully,

J. MURRAY.

P. S. I take the liberty of pointing out a slight oversight in the statement in your last number (pages 43 and 44) with regard to my paper on Mineral Waters. It is mentioned that, in the opinion I had advanced of muriate of lime and sulphate of soda being present together in a mineral water, I had been anticipated by Pfaff, who had stated muriate of lime and sulphate of magnesia as

ingredients of sea-water; and as his dissertation was published in Schweigger's Journal, September, 1814, the anticipation, it is remarked, is at least that of a year. Though the volume of the Transactions of the Royal Society of Edinburgh, in which my paper appeared, was published in June, 1815, yet the paper was read on the 20th of November, 1814; and the analysis itself was executed in August and September, so that, strictly speaking, there cannot be said to have been any anticipation. Besides, there is no novelty in the mere observation that muriate of lime, and sulphate of magnesia, or sulphate of soda, may exist together in a mineral water, for this has often been advanced, and has always been ascribed to the circumstance to which it is referred by Pfaff; the state of great dilution. The novelty of opinion consists in the inferring that these salts are the ingredients of a mineral water, from the obtaining by its analysis muriate of soda, or of magnesia, and sulphate of lime, and of course, regarding these latter not as original ingredients, but as products of the operation. Of this Pfaff seems to have had no idea, and could not indeed have had, as the account which he gives of the composition of sea-water is incompatible with it. He states sulphate of lime as an ingredient, as well as muriate of lime, which he would not have done, had he had any conception of the above opinion. His statement of the composition of a mineral water, which you give in the same page, is equally incompatible with it. The ingredients of that water, according to the view I have given, are carbonate of soda and muriate of lime, and not, as he states them, carbonate of soda, muriate of soda, and carbonate of lime.

Article VII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday the 25th of January a paper by Sir Humphry Davy was read, containing further experiments on the effect of wire sieves to prevent the combustion of gases from passing through them. A sieve formed of wire $\frac{1}{50}$th of an inch in diameter, and containing 10 wires in the inch, prevented the combustion from penetrating; but when agitated in an exploding mixture explosion took place. The explosion likewise took place when the wire became red-hot. When there were 14 wires in the inch agitation did not occasion an explosion. With 24 wires to the inch, the mixture did not explode even when the wire became red-hot. The author accounts for these singular phenomena in this manner. A red-hot wire of a considerable size is required to produce an ex-

plosion in gases; hence, when the wire is very small, explosion does not take place, even when the wire becomes red-hot. The gas will not explode till it acquires a certain temperature. Now in the experiments with the wire sieves this temperature only takes place at the top, where the gas is so much dilated with azote and carbonic acid gas, that it is incapable of exploding.

At the same meeting a paper by Dr. Wilson Philips was partly read, containing experiments on the nervous influence in secretion. In two former papers he had shown that the circulation of the blood and the action of the muscles were independent of the nervous influence, and that this influence only acted on the muscles like any other stimulus. But the case is very different with the secretions. Whenever the nervous influence is interrupted the secretion is at an end. Several rabbits had the eighth pair of nerves divided, and in all of them the parsley, which they ate after the operations, remained in the stomachs quite unaltered, and exactly resembled parsley chopped small with a knife. The stomach was always much distended, and a portion of the food was contained in the œsophagus. This was owing to the unsuccessful attempts which the animal made to vomit, which always follow the division of the eighth pair. The animal soon shows a violent dyspnæa, and seems to die at last of suffocation.

Since the experiments of Galvani on animals, it has been a favourite opinion of many physiologists that the nervous influence is the same with galvanism. To put this to the test of experiment, a portion of the hair of a rabbit opposite to the stomach was shaved, a shilling tied on it, the eighth pair was divided, and the extremities of the nerve coated with tinfoil. These were connected with a galvanic battery of 47 pairs of plates four inches square. The trough was filled with a liquid composed of one part muriatic acid and seven parts water. This action was kept up for 26 hours. No dyspnæa took place, and after death the food in the stomach was found as much digested as in the stomach of a healthy rabbit which had eaten food at the same time. The smell of the parsley was destroyed, and the smell existed which is peculiar to the stomach of a rabbit during digestion. This experiment was several times repeated with the same result. So that it appears that the galvanic energy is capable of supplying the place of the nervous influence, so that while under it the stomach digests food as usual.

Mr. Wilson likewise made a number of experiments to show that heat is a secretion from the blood produced by means of the nervous energy. When new drawn blood is subjected to the action of the galvanic battery, it continues several degrees hotter than blood not subjected to the same process.

It appears to me that Mr. Wilson has gone rather farther than his experiments will warrant, when he concludes that the nervous influence and galvanism are the same. It is clear that the section of the nerve interrupts the nervous influence. Mr. Wilson's experi-

mets (supposing them correct) show us that galvanism puts an end to this interruption. But it may do this merely by serving as a conductor to the nervous influence.

On Thursday, the 1st of February, Dr. Wilson Philips' paper was continued: he considers it as proved by his experiments that the ganglia communicate to the nerves proceeding from them the general influence of the brain and spinal marrow. Nerves proceeding from them supply all the involuntary muscles. But if this be the case, it will be asked, how comes the digestive power of the stomach to be destroyed by cutting the eighth pair of nerves, seeing that the stomach is supplied with nerves from ganglia? The eighth pair coming from the largest portion of the nervous matter possesses the greatest influence; but the digestive power of the stomach is weakened likewise by the interruption of the nerves proceeding from ganglia. This he proved by destroying part of the lower portion of the spinal marrow of different rabbits. In every case the digestive power of the stomach was impaired or destroyed; the urinary bladder and rectum lost the power of discharging their contents, and paralysis of the lower extremities ensued, and a great degree of cold took place. The heat of one rabbit before death sunk as low as 75°. Though the power of the stomach as an organ of digestion is destroyed by cutting the eighth pair of nerves, still its muscular power remains; but it does not act as usual, because the stimulus of digested food is wanting; or it acts so as to throw the food out of the stomach the wrong way, in consequence of the unnatural stimulus of undigested food.

On Thursday, the 8th of February, Dr. Wilson Philips' paper was concluded. He shewed that the heat of animals was in all probability owing to the nervous energy. He finished his paper with a general view of the facts which he had established in the three papers which he had laid before the Royal Society. The muscular energy depends upon the particular structure of the muscles; the nervous system is supported by the sanguiferous; but the sanguiferous can act without the influence of the nervous system. Secretion and animal heat are entirely dependent upon the nervous system. Hence the muscles cannot for any length of time continue to exert their energy if the nervous influence be cut off. The nervous influence appears the same with the galvanic energy.

At the same meeting a paper by Dr. Brewster, on the structure of the crystals of fluor spar and common salt was read. Haüy had observed that all minerals whose primitive forms were symmetrical, as in the cube and tetrahedron, refract singly; these figures belong to fluate of lime, common salt, alum, &c. Biot first attempted to give a reason for this curious circumstance. He observed that doubly refracting crystals act upon light two ways; some draw it nearer the axis, while others repel it to a greater distance: the first exert an attraction; the second kind a repulsive force. The crystal of fluor spar, &c. according to Biot, are intermediate between the other two, and therefore neither attract nor repel. Dr. Brewster found that

crystals of fluor spar and common salt, in certain cases, depolarize light, in others not. Whenever any deviation from the exact figure of the crystal takes place, they acquire the power of depolarizing; and this deviation may be either towards the side of attraction or repulsion.

On Thursday, the 15th of February, a paper by Mr. Tod, surgeon in the navy, was read, containing some experiments and observations on the torpedo electricus. While the ship Lion, in which Mr. Tod was, lay at the Cape of Good Hope, a considerable number of this fish was caught by the seine, but none by the line, though they fished with every kind of bait in the very place where the fish was caught by the seine. When caught it was put into a tub of water, where it lived usually three, and in one case, five days. Mr. Tod in the paper gives a description of the fish, and mentions its general size, which is known to be small. The fish discharged the electric energy at pleasure, and it ceased with the life of the animal. When caught it attempted in the first place to make its escape by muscular exertion, and did not exert its electric energy till it found the first attempt unsuccessful. A motion of the eye of the fish was generally perceptible when it exerted this energy, so that Mr. Tod could generally tell when the shock was given to another person who held the animal in his hand; the shock never reached farther than the shoulder, and often not farther than the elbow.

At the same meeting, two papers by the Rev. Abram Robertson, D.D. F.R.S. were announced: the first giving a method of calculating the excentric from the mean anomaly of a planet; the second containing a demonstration of Dr. Maskelyne's method of finding the longitude and latitude of a celestial object from its right ascension, and *vice versâ*; and pointing out two mistakes to which Dr. Maskelyne's method is liable.

On Thursday, the 22d of February, a paper by Sir Everard Home was read, giving an account of the mechanism of the feet of an East Indian species of lizzard, which is capable, like the common fly, of walking up and down the perpendicular face of smooth walls without falling. This mechanism consists in a particular muscular contrivance, by which a quantity of air contained between the wall and the foot of the animal, enclosed within a kind of cartilaginous ring, is rarefied so much as to enable the foot of the animal to adhere with sufficient force to support the whole weight of the body. This rarifaction is continued without any muscular exertion after it has once taken place. There can be little doubt that the foot of the fly is constructed in the same way, though its size is so small that nothing can be determined by inspecting it with the naked eye. When high magnifying powers are employed, the observer is so liable to be deceived by appearances that nothing very precise can be determined on the subject.

LINNÆAN SOCIETY.

At the meetings of the society on the 6th and the 20th of Fe-

bruary, a paper was read by Robert Brown, Esq. Librarian to the society, containing general observations on the tribe of plants called Compositæ. This paper contained many curious remarks upon the structure of the flowers of this difficult tribe of plants, marked by the precision and sagacity which characterize all the papers of this acute observer; but of so miscellaneous a nature that it is scarcely possible to give any abstract of it without transgressing our usual limits.

ROYAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Royal Institute of France during the Year 1815.

PHYSICAL DEPARTMENT.—*By M. le Chevalier Cuvier, Perpetual Secretary.*

Another year of devastation and terror! The bloody discord of our new country, the existence of this fine kingdom brought into doubt, the repose and the fortune of the most peaceable citizens for some time without protection or security; innumerable armies inundating our provinces, taking possession of our towns, seizing by violence in the midst of a conquered capital those treasures of the arts formerly collected in a manner equally violent. Such, to the most innocent, have been the consequences of a too culpable attempt. But the sciences bring with them a state of consolation and tranquillity. At present all nations respect them. In the midst of the tumult of arms, our Archimedes have nothing to fear from enlightened soldiers, to whom their names and their labours are known, and who rejoice to become for a short time their disciples. It is perhaps even in the most terrible moments that, taking refuge in profound meditation, emancipating themselves by the exaltation of their minds from the horrors that surround them, they have made some of the most fortunate combinations, and some of the most fruitful discoveries. We shall find, at least, that the list of the labours of this year is not inferior to that of the most peaceable time.

CHEMISTRY.

We have been speaking for two years of those acids without oxygen, or, as they are now called, *hydracids*, which have made so considerable a breach in the imposing chemical edifice of Lavoisier. The labours of Gay-Lussac have shown this year that there is one more to add to this class—the acid called *prussic* by M. de Morveau, because it enters into the composition of Prussian blue; and its radical not being then known, it was not possible to give it a name from that substance.

The experiments of Margraaf, Bergman, and Scheele, demonstrated that in Prussian blue the iron was united to a substance which acted the part of an acid. Berthollet had long suspected that no oxygen entered into its composition, but merely carbon,

azote, and hydrogen. The truth of this suspicion has been ascertained by Gay-Lussac.

On decomposing, with the precautions which he indicates, the prussiate of mercury by muriatic acid, he obtained pure prussic acid; and we have already in one of our preceding reports spoken of the singular properties which he ascertained it to possess in that state, particularly its great volatility. Burning this vapour by means of oxygen and the electric spark, he obtained determinate quantities of water, carbonic acid, and azote. He abstracts the oxygen consumed in the formation of the first two bodies, and obtains this conclusion, that one volume of the vapour of prussic acid results from the combination and concentration of one volume of the vapour of carbon, half a volume of azote, and half a volume of hydrogen; or expressing these volumes in weight, according to the density of the vapours, 100 parts of the acid contain,

Carbon	44·39
Azote	51·71
Hydrogen	3·90
	100·00

This prussic acid contains more azote and less hydrogen than other animal substances, from which it is particularly distinguished by the absence of oxygen.

This is the first known hydracide with a decomposable base. Gay-Lussac has likewise succeeded in obtaining this radicle in a separate state. The accidental name *prussic* not being proper, he has given it the name of *cyanogen* (that is, *producing blue*). Henceforth prussic acid must be called *hydro-cyanic acid*; its saline compounds, *hydro-cyanates*; and the compounds of its radicle, *cyanurets*.

We wish it were in our power to give an account of the numerous and delicate experiments by which Gay-Lussac has reduced under the one or the other of these classes the different products of the action of prussic acid upon bodies, and the properties which he has recognised in them. Let it suffice to say that Prussian blue rather appears to him a cyanuret of iron containing water than a *hydro-cyanate*, or to use the old name, a *prussiate*.

Cyanogen itself possesses peculiar properties. It is a permanently elastic fluid, of the specific gravity 1·8064, having a peculiar and strong odour, giving a sharp taste to water, and burning with a purple flame. Water absorbs four times its volume of it, and alcohol 23 times its volume. Its direct analysis gave the same results as that of hydrocyanic acid, namely one volume of vapour of carbon, and half a volume of azote.

Gay-Lussac has likewise presented to the Class, memoirs on the cold produced by evaporation, and on evaporation in air of different degrees of temperature and density, in which he expresses by a formula the result of his experiments. He has, likewise, given a

memoir on hygrometry, containing the immediate consequences of these experiments; but these works not having, in his opinion, acquired that precision and order which he is accustomed to give to all that he publishes, the author has thought proper to defer the printing of them.

M. Dulong, Professor at Alfort, has presented some experiments on oxalic acid, which, though not constituting a complete work, open interesting views for the science. When this acid is saturated with barytes, strontian, or lime, we obtain always salts, which represent the acid employed even when they have been exposed to a heat higher than that of boiling water. But with oxide of lead, or of zinc, we always lose 20 per cent. of the acid by drying. When these metallic salts are afterwards strongly heated, no water makes its appearance; but we obtain carbonic acid and carbonic oxide, and there remains behind the oxides of the metals employed, of which that of lead possesses particular properties. The oxalates of copper, silver, and mercury, on the contrary, always give out water when decomposed, how dry soever they are previously made. Carbonic acid is likewise given out, and the base remains in the metallic state. The oxalate of silver detonates, and we know already that it detonates when struck, as well as the oxalates of mercury.

As to the oxalates of barytes, strontian, and lime, they give, when decomposed by heat, empyreumatic oil, water, carbonic oxide, carbureted hydrogen, carbonic acid, and there remains a mixture of subcarbonate and charcoal.

These phenomena may be explained two ways. Oxalic acid is either composed entirely of carbon and oxygen, in proportions intermediate between those of carbonic acid and carbonic oxide; but it contains water, which certain oxalates, as those of lead and zinc, lose when dried; while others retain it. Or it is a compound of carbonic acid and hydrogen. This last constituent, with the oxygen of the oxide, will form water, which these first oxalates likewise allow to escape, and nothing remains but carbonic acid and the metal, a combination quite new in chemistry: for it is regarded as a general principle, that metals are capable of uniting with acids only after being oxydised. M. Dulong, who is inclined to this last explanation, conceives of course, that the dried oxalates of lead and zinc are not real oxalates, and he proposes to give to them, as well as to similar compounds that may be discovered, the name of *carbonides.* The oxalates which do not give water by drying, contain the oxalic acid entire; and as from its composition it will be named hereafter *hydro-carbonic,* the salts will take the the name of *hydro-carbonates.*

M. Dulong is led by analogy to very general conclusions, by which he reduces under the same laws, not only the ordinary acids, but likewise the hydracids. But we shall give a more detailed account of his opinions, when he sends up the memoir in which he intends to consign them.

The chemical action of solar light on bodies is worthy of all the

attention of philosophers, from its influence on most of the phenomena of living nature, yet it has hitherto been but little examined. M. Vogel has just added some experiments to those which we formerly possessed. Ammonia and phosphorus, which do not act on each other in the dark, when exposed to the solar light disengage phosphoreted hydrogen gas, and deposit a black powder composed of phosphorus and ammonia intimately united. Nearly the same thing takes place with phosphorus and potash. The action of the different rays is not always similar; the red rays produce no effect on the solution of corrosive sublimate in ether, while the blue and complete light produce a mutual decomposition. The metallic permuriates are brought in the same way to the state of protomuriates.

We have said a few words in our two last reports on the researches of M. Chevreul, assistant naturalist to the Museum of Natural History, concerning soap and saponification. This skilful experimenter has ascertained that the action of potash on tallow produces new modes of combination, from which result substances which did not exist before perfectly formed, and two of which, margarine and a species of fluid oil, acquire all the properties of acids. The author, pursuing his experiments, has ascertained that the same effects are produced by soda, the alkaline earths, and different metallic oxides, and that the resulting substances are in the same proportion, whatever agent we have employed. Magnesia and alumina on the contrary merely contract a certain union with tallow, without separating its elements into two distinct bodies. The quantity of alkali necessary to convert a given portion of tallow into soap is exactly that which saturates the margarine and oil which the tallow produces. Our laborious chemist has terminated his memoirs on this subject, by giving the capacity of saturation of margarine and fluid tallow, and by describing the properties of several new soapy combinations which he produced by double decompositions, by mixing a hot solution of soap, of fluid tallow, and potash, with different earthy and metallic salts. Thus he has rendered the soaps, the study of which has been hitherto neglected, almost as well known as the salts with which chemists have been the most occupied.

The late M. Fourcroy made known, under the name of *adipocire*, a substance separated by means of acids from the fatty matter into which animal bodies buried in the earth are converted. And he considered it as identical with the crystalline matter in human biliary calculi, and with the spermaceti found abundantly in certain cavities of the head of the cachalot.

M. Chevreul, led by his experiments to examine these substances, has found that the crystalline matter of biliary calculi does not form soap, while spermaceti furnishes it as easily as tallow; but producing a somewhat different alteration in other proportions and with particular properties. The fatty matter of dead bodies is much more compound than Fourcroy had supposed, containing different fatty bodies combined with ammonia, potash, and lime. It is a fatty matter that has already experienced the action of alkalies.

Every person must have observed a resinous excretion of a yel-

lowish-orange colour, which exudes from cracks in the bark of beech faggots exposed to moisture. It has the shape of ribbons, twisted like vermicelli. M. Bidault de Villiers has made some chemical experiments on this matter. One portion of it dissolves in water, another in alcohol, and the residue possesses some of the properties of gluten. Nitric acid converts it into oxalic acid, into a yellow bitter principle, which is very abundant, and into a fatty matter; but produces no saclactic acid. When heated it gives abundance of carbonate of ammonia, and a fetid oil; so that the Commissioners of the Class were led to consider it as approaching very closely to an animal substance. It would be interesting to inquire into the cause of its production.

One of the periods in which chemistry has shown itself most brilliant and most useful, was certainly that in which France, separated for 20 years from countries whose productions had been considered for so long a period as real necessaries, was obliged to supply them by the products of its own soil. The known arts have been perfected and new ones created. We have seen in succession soda extracted from common salt; alum and copperas formed by uniting their ingredients; colours considered as fugitive rendered permanent; indigo from woad supplying that from the indigofera; madder supplying the place of cochineal; and sugar from beet employed as a substitute for that from the sugar cane.

This last article, the most important of all, is far from having lost its interest even at present. Many of the manufactories, indeed, have fallen; but those which were properly conducted still subsist and prosper; and according to M. le Comte Chaptal, their product will always be able to rival the sugar of the colonies. This skilful chemist gives an unanswerable proof of his assertion by continuing to manufacture with profit. It is true that in all the details of the culture, harvest, and preparation, and likewise in the employment of the different waste matters, he has applied all the lights of science and experience, so as never to throw away what can be of any service, and to apply to other uses what he is obliged to reject. He has described his processes in a manner sufficiently clear to be understood by all the manufacturers, and we have reason to hope that his work will assist in preserving to France a precious manufacture, which a thousand events may again render necessary to the country.

The third volume of the Elementary Chemistry of Thenard has been published. This skilful Professor describes in it with great minuteness, and according to the most recent discoveries, for many of which the science is indebted to himself, the immediate principles of organized bodies, the different products of their decompositions, and their uses in the arts. The fourth, which is in the press, will terminate the work.

(*To be continued.*)

Article VIII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

Mr. Clarke will commence his next Course of Lectures on Midwifery, and the Diseases of Women and Children, on Monday, March 18. The lectures are read every morning, from a quarter past ten to a quarter past eleven, for the convenience of students attending the hospitals, at No. 10, Saville-row.

II. *On taking Specific Gravities, and on the Cause of the Rupture of Leaden Pipes from Frost: in an Extract of a Letter from Dr. Redman Coxe, Professor of Chemistry, Philadelphia.*

It occurs to me, before I close my letter, to mention what I consider as a source of error in determining the specific gravities of bodies; this is the employment of water as a standard at any other temperature than that at which this fluid is of a mean density, viz. at about 40°. If both above and below that point this fluid expands both by an increasing and diminished temperature, ought we not to fix upon that degree invariably for the purpose? for as we judge by comparative bulks of matter, a little variation may induce considerable difference of result. Another circumstance arising from this anomaly in water expanding by cold below 40° is, that this is the chief cause of leaden pipes bursting, and not from the mere sudden expansion in its conversion into ice. My reason for believing this to be the case is, that we never see the leaden tube burst throughout the whole length, as should be the case if the solidification of the water was the cause; for as this is uniform throughout, so ought the effect to be: but it is chiefly in a small point, arising, as may be seen, from the gradual diminution of thickness in that part of the pipe, until becoming as thin as paper, it can no longer sustain the pressure of the fluid, and therefore yields. Hence only a few small openings are perceptible, not bigger than pins' heads, which takes off the pressure by giving vent to the water. If this is the case, how can it be guarded against, excepting by increasing the thickness of the tube? Now as the water goes into the pipe in a fluid state, it can scarcely become colder below the earth, removed as it is from the influence of the frost. This is a circumstance worthy of consideration in large cities where leaden pipes are employed for the distribution of water. Did you ever hear of rats gnawing the leaden pipes to get to the water within? I have a large specimen of this kind.

III. *Chyle.*

Dr. Marcet has published a set of comparative experiments on the chyle from vegetable and animal food, in the sixth volume of

the Medico-Chirurgical Transactions. The chyle was collected from the thoracic duct of dogs within three hours after they had been fed. The following were the results obtained:—

1. *Chyle from Vegetable Food.*—It appeared a short time after being collected, in the form of a semi-transparent, inodorous, colourless fluid, having a very slight milky hue, like whey diluted with water. It contained a coagulum, which was semi-transparent, and resembled the white of an egg, but had a pink hue. The weight of the fluid part to that of the coagulum was 100 to 48; but the coagulum being put into a phial by itself, a quantity of fluid similar to the serous portion speedily oozed out, and left only a very small clot. This residue began to putrefy at the end of a week. Potash caused a slight ammoniacal smell to exhale from the fluid which issued from the clot; and the mineral acids, especially the nitric, precipitated abundance of white flakes, which were partly redissolved by dilution and heat.

The serous portion had the specific gravity 1·0215 or 1·022. It did not putrefy in ten days, but acquired a smell similar to that of sour cream. Heat rendered it somewhat turbid and milky. The mineral acids threw down abundance of albumen: 100 parts of it being evaporated to dryness, left 4·8 parts of a yellow and very deliquescent solid residuum. Some other portions yielded more solid matter. The greatest quantity was 9·5 per cent.

2. *Chyle from Animal Food.*—It was white and opake, like cream. The coagulum was white and opake, and had a more distinct pink hue. The proportion of the fluid to the coagulated portion was 100 to 46·5. This coagulum, like the preceding, gradually gave out a serous fluid, till only a very small quantity of matter remained. This residue, somewhat similar to thick pus, became putrid in three days.

The serum, on standing, formed an opake creamy substance on its surface. Heat rendered it more turbid than the preceding serum. It contained abundance of albumen. The quantity of solid matter contained in it was seven per cent.

These liquids, when distilled, gave out moisture, carbonate of ammonia, and a heavy fixed oil. Vegetable serum left three per cent. of charcoal; and animal serum, one per cent. The presence of iron was recognized in the residuum, and the same proportion of salts (about nine in the 1000 parts) that exist in animal fluids in general.

IV. *Chyme.*

Dr. Marcet likewise examined chyme from the stomach of a turkey. It was a homogeneous, brownish, opake pulp, having the smell which is peculiar to poultry. It was neither acid nor alkaline, and became putrid in 12 days. When evaporated to dryness, it left nearly one-fifth of its weight of solid matter. It contained albumen. When burnt, it left 12 parts in the 1000 of charcoal. This residuum contained iron, lime, and an alkaline muriate.

V. *On Nitrate of Silver as a Test of Arsenic.*

An ingenious student at Guy's Hospital suggested to Dr. Marcet that when nitrate of silver is mixed with a solution containing an alkaline phosphate, a yellow precipitate is thrown down, similar in appearance to arseniate of silver. Hence an ambiguity in the mode of detecting arsenic in the liquid contained in the stomach, where an alkaline phosphate may very well be present. This fact induced Dr. Marcet to examine the subject anew. The shade of the two salts is not quite the same. Yet in juridical cases other tests may be requisite to be assured of the presence of arsenic. The addition of sulphate of copper and potash, and the formation of Scheele's green, affords a very satisfactory confirmation. But the best mode of proceeding is to mix the supposed arsenite of silver with a little potash and charcoal powder, and expose it to heat in a glass tube. A pellicle of metallic arsenic will be obtained on the inside of the tube, unless the quantity of arsenic present be very minute indeed.

VI. *Query respecting the Use of the Liver.*

(To Dr. Thomson.)

SIR,

In the valuable and much admired sketch of the improvements in physical science, with which you presented your readers this month, I met with the following paragraph, which contains an inference the accuracy of which does not immediately appear. This is the passage:—

"From the discovery of Mr. Rose that urine in hepatitis contains no urea, I think it may be inferred that one use of the liver, *if not the only one*, is to separate urea from the blood; so that it would seem to be the principal organ concerned in the formation of urine."

I doubt not but the conclusion appears perfectly satisfactory to your own mind, and would probably appear equally so to the minds of your readers, if you would be rather more explicit in the statement of the grounds from which this very important deduction is drawn. Will you excuse my liberty in requesting you to satisfy me and others of your readers by adverting to this subject in the next number of your *Annals of Philosophy.*

Edinburgh, Jan. 11, 1816. Φιλος.

In hepatitis the liver is diseased; therefore I infer that it is incapable of performing its usual function. The disappearing of urea being a constant concomitant, I infer that the function of the liver in its sound state is to secrete urea.

VII. *Queries respecting Ginger and Gas Light.*

(To Dr. Thomson.)

SIR,

It is a fact no doubt known to many of your readers, that when a small quantity of powdered ginger is stirred into ale or beer

slightly warmed, a brisk effervescence ensues. To what is this effect to be ascribed?

I beg to know what would be the probable result if a fire were to destroy one of the large gasometers belonging to the Gas Light Company when full of gas. Would not the consequence prove almost as destructive to the neighbourhood as if a large quantity of gunpowder were to be exploded?

Can the gas light be safely adopted by a silversmith? Would not the sulphureous smell, which was the objection to its continuance at the Theatre, be likely to produce a discoloration of his silver articles?

London, Jan. 13, 1816. INQUISITOR.

To the first of these queries I am unable to give any answer, as I have had no opportunity of witnessing the fact. Perhaps some of my readers may be able to give a satisfactory explanation of it.

Carbureted hydrogen gas possesses no resemblance to gunpowder, as it will not burn at all unless mixed with more than six times its bulk of common air. A gasometer filled with the gas, if set on fire, would not explode; but the flame would probably be so violent that it would set fire to the house in which it was placed.

If coal gas containing sulphur be mixed with the air of a room unburnt, it would doubtless tarnish silver. But it would not produce this effect if it were burnt. I think the burners might be so contrived, by placing the stop-cock near their mouth, that the whole gas shall be consumed. In that case there would be no smell, nor would any injury be sustained by silver.

VIII. *Queries respecting the Mode of Cutting Glass.*

(To Dr. Thomson.)

SIR,

In Dr. Henry's Elements of Chemistry, we find directions for cutting off phial bottles, by winding a thread or string round them previously dipped in oil of turpentine, and then inflaming it. Following these instructions, we have never been able to succeed. As a good process for cutting down oil flasks, to make evaporating dishes, would be extremely convenient, perhaps you will oblige us by communicating a successful method.

Jan. G. K. and M.

Answer.—I have never myself tried the method alluded to in this letter. My process is this. I take a bar of iron (a poker, for example), and heat its extremity red-hot in the fire. I then take advantage of any crack which previously exists in the retort or flask to be cut; or if no crack exists, I make one by heating a portion of the edge pretty hot by means of the iron bar, and then touching it with a drop of water. This crack is readily extended in any direction you choose by placing the extremity of the red-hot bar a little before it. The crack speedily extends to the bar. The bar is then withdrawn a little further on. In this manner you proceed till you

have cut out an evaporating dish of the shape required. A little practice will enable the operator to cut the glass neatly and readily.

Lavoisier's method was to surround the flask or retort at the part to be cut with a red-hot iron ring, and then to wet the part. This method is probably good enough (indeed I have sometimes used it); but it would require a separate ring for every size of flask or retort to be cut. On this account it is inferior to the method which I have described.

IX. *Query respecting the Mode of removing common Putty from Glass.*

(To Dr. Thomson.)

SIR,

I have read somewhere of a chemical preparation or process by which the putty that fastens the squares of glass in a window can be decomposed, or its adhesive property destroyed, so as to permit removal of the glass without the risk of breaking. If you, or any of your Correspondents, will be so kind as to communicate the method by which this is accomplished, or point out any publication from which the information may be obtained, it will very much oblige

Arbroath, Jan. 27, 1816. A CONSTANT READER.

Answer.—Common putty is nothing but a paste made of chalk and lintseed oil. Hence it is readily softened and removed by the application of any acid. Nitric acid will act most speedily; but muriatic acid may likewise be used. Indeed, if no other acid be at hand, the putty might be removed from the glass by vinegar, but the process would be tedious. Even alkaline leys would have considerable effect by acting upon the oil, though they would not answer so well as acids.

X. *Greywacke to the North of the Forth.*

(To Dr. Thomson.)

DEAR SIR,

On reading your very highly interesting Account of the Improvements in Physical Science during the Year 1815, in the *Annals of Philosophy* for this month, I observe, under the head of *Geognosy* (p. 65), the following sentences: "Next to the primitive come the class of rocks called *transition.* They contain petrifactions, and are very abundant in Great Britain. I do not know that they have been observed further north than the Frith of Forth."

You will, I know, excuse me when I put you in mind of what you have, amid your multifarious engagements (and no wonder), happened to overlook. I allude to a short paper of mine, entitled, Mineralogical Observations in the Highlands of Scotland, obligingly published by you in the *Annals of Philosophy* for July, 1813, Art. VIII. By turning to that article, you will find that in August, 1812, Mr. Iardine and myself discovered greywacke north of the Frith of Forth. Near the beginning of the paper referred to are

these words, which I beg leave to quote: "From Callendar we set out to visit the famed and interesting scenery of the pass of Leney, by which the traveller on this route enters the Grampian range. About two miles beyond Callendar we found the rock through which the road is cut to be very distinct greywacke, and traced it till we found it about half a mile further on towards the north-west, very near the mica-slate; but could not see the junction of these two rocks, or whether the clay-slate intervened between them. We were both perfectly satisfied that in this district the transition rocks, greywacke, and greywacke-slate, come in between the floetz and the primitive country." And again in the same paper (p. 29), "In the evening of the 16th we visited the fall at Bracklin bridge, about a mile to the east of Callendar. The rock is conglomerate, and broken down by the action of the water into many fine and fantastic forms. We had the conglomerate all the way from Callendar to this fall; and on tracing the river about two miles up, observed no other rock; but Mr. Iardine told me that some time ago Sir James Hall found greywacke about a mile or two higher up than we were. Night prevented our reaching it."

"The greywacke and greywacke-slate appear about two miles after leaving Callendar, on the road to Loch Catherine. They continue all along the valley of Loch Venachar and Loch Achray to the Trossacks, and the eastern part of these hills is composed of greywacke.

I am, with great esteem, dear Sir, yours very truly,

Cockpen, Jan. 22, 1816. JAMES GRIERSON.

XI. *Intended Publication on Greenland.*

Mr. Wm. Scoresby, jun. has in the press a work which he proposes to call the History of East and West Greenland, and of the Northern Whale Fisheries.

The author of this work, having been in the habit of annually visiting the Greenland seas, since the year 1802, began about 10 years ago, for his own amusement, to make memoranda of the various natural phenomena with which this country abounds. Finding his notes rapidly increase, both in interest and variety, as well as in bulk, and observing that they contained a collection of facts, which must be in a great measure unknown to the world in general; considering, at the same time, the singular barrenness of information on subjects of such general interest and national importance as the History of the Greenlands, and of the Northern Whale Fisheries, he was induced to undertake the work, a prospectus of which is now respectfully submitted to the public.

It is well known, that no book in the English language has yet been devoted to the same objects with the work now announced, since the time of Egede and Crantz; and these authors merely mention the whale fisheries in a cursory manner, and neglect altogether their establishment and history. The French and Dutch, meanwhile, have each issued different works expressly on the sub-

ject in hand; some of these being in the possession of the Author, he has availed himself of whatever information is interesting in the historical and other parts, which his own observations are not calculated to supply. His materials have thus become ample.

The work will be illustrated by a variety of engravings, consisting of maps, plans, and sketches.

The maps, comprising delineations of East and West Greenland, will be improved from an original survey of the greater part of the west coast of the former, wherein many gross errors in the charts extant will be rectified. The plans and sketches will include representations of the various instruments used in the whale fishery, amounting to more than 40 articles:—the appearances of the land, ice, crystals of snow,—whales, narwhales, walrus, &c.—some birds,—a variety of mollusca,—together with views of the fisheries, &c.

The following are the principal subjects that will be treated of:—

I. An account of the progress of discovery in the north, with a synopsis of the numerous voyages undertaken in search of a northern passage to India.

II. An account of West Greenland:—its extent, appearance, natural history, aborigines, colonies, manners, and customs of the inhabitants, &c.

III. East Greenland, or Spitsbergen:—its appearance, natural history, harbours, icebergs, mountains, colonisation, products, &c.

IV. The natural history of the Greenland seas; containing,

1. An account of the Greenland sea:—its situation and extent, singular varieties of colour, occasional transparency and frequent opacity, temperature both at the surface and at considerable depths, currents, tides, depth, &c.

2. The polar ice:—its varieties and properties, mode of generation, &c.; its extent, situation, and variation; with a comparison of the degree of approximation towards the poles, attained by various navigators, in different meridians; and a demonstration of the possibility (contingencies excepted) of performing a journey over the ice to the north pole.

3. The atmosphere: its peculiarities, such as surprising refractions, &c.:—its changes of pressure, as shown by the barometer, frequently sudden, great, and portentous, &c:—its temperature, mean, monthly, and annual, range of temperature, probable temperature of the north pole, cold, and its effects, &c.:—winds, their variableness, astonishing changes both in intensity and direction, duration and frequency of storms in the spring of the year, &c.:—meteors, clouds, snow, and its beautiful crystallisations, hail, frost-rime, Aurora Borealis, &c.

4. The zoology:—the whale, and its various genera;—the walrus, seal, bear, &c.:—birds:—some curious varieties of nondescript mollusca, and other marine animals, and animalculæ, &c.

V. The history of the northern whale fisheries, from the earliest records to the present time; showing, the progress of this art, and its singularly great advancement, with a clear account of those

principles on which a successful fishery depends:—comprising, likewise, an account of the construction of a ship which seems best adapted for this trade, the mode of its equipment, with a statement of expenses, and a description of the boats, instruments, and apparatus, of the most improved principles with which it is furnished; together with a view of the modern method of discovering and attaining the haunts of the whale, effecting its capture under every variety of circumstance; and, a selection of anecdotes illustrative of the dangers of this occupation, and of the singular accidents which sometimes occur.

VI. The history of the minor fisheries:—for seals, walruses, &c.:—with the method of killing these and other animals, inhabitants of the Greenland seas.

VII. A journal of a Greenland whale fishing voyage.

VIII. Appendix; containing, an extensive series of meteorological tables, from which are deduced some important facts, relative to the temperature and pressure of the atmosphere, prevailing winds, &c.:—interesting tables of meteorological results:—tables of the variation of the compass, latitudes, and longitudes, &c. from original observations.

Greenland captains, or other gentlemen, who have met with remarkable adventures in the whale fisheries; or who, from research or observation, may be able to supply information calculated to add to the interest of this work, will, by sending an account thereof to the author at Whitby, confer a particular obligation on him.

XII. *Heat from Friction.*

Though the ascent and descent at Blackfriars bridge be very inconsiderable, it is always customary to fix a drag upon one of the wheels of the heavy waggons when they cross it. One day towards the end of January, as I happened to cross this bridge, I met five or six waggons all heavily loaded, and a wheel of each as usual fixed by the drag chain. The day before had been rainy, and the bridge had that forenoon been swept by the scavengers; the pavement, however, was still very wet, though not covered with deep mud. The drag wheel of the first waggon that I met left the tops of the the stones dry, and a train of smoke rose after it nearly as strong as rises from boiling water, so that it was visible at a considerable distance; this was also the case with the drag wheel of all the other waggons, the smoke was so conspicuous that it drew the attention of a boy who acted as drayman to one of the waggons; for I observed him following the drag wheel, and feeling the stones with his hand to determine whether they were heated. I conceive the heat of the iron rim of the wheel, when dragged along the ground, must have been considerably greater than that of boiling water, for in an instant (while dragged along the ground at the ordinary rate) it heated the water in its way so as to make it smoke very strongly. Here the waste of heat must have been very great, as the same spot of the wheel came continually in contact with water not much higher than

the freezing temperature. I consider this fact as scarcly less striking than Count Rumford's experiments on the heat evolved by friction at Munich.

XIII. *St. Helena.*

The late Dr. Roxbourgh while at St. Helena, where he spent several months, drew up a flora of that island. He found in it 56 species, 50 of which were peculiar to the island, having been observed no where else. Not a single new genus occurred.

XIV. *Prizes of the French Institute.*

The prize for the best set of physical experiments during the course of 1815 was divided between M. Seebeck and Dr. Brewster.

The prize for the mathematical theory of the vibrations of elastic surfaces, and the comparison of them with experiment, was given to Mademoiselle Sophie Germain, of Paris.

The prize for the theory of waves at the surface of a gravitating fluid of an indefinite depth, was given to M. Augustin Louis Cauchy, Ingeneur des Ponts-et-Chaussées.

Lalande's medal was voted to M. Mathieu, an astronomer attached to the Royal Observatory of Paris.

XV. *Cinnamon Stone.*

Specimens of the rock containing the cinnamon stone of Werner have been brought to London from Ceylon. It consists of three constituents: namely, schalstone, quartz, and cinnamon stone. The schalstone constitutes the principal ingredient, and has the usual imperfectly foliated appearance, and all the characters which distinguish the variety of it found in the Bannat of Temeswar. The quartz is distributed irregularly, and has no appearance of crystallization. The cinnamon stone is in grains, none of which exhibit any traces of a crystalline form. I observed one of the grains, indeed, which bore some resemblance to the garnet dedahedron; but the apparent faces were conchoidal, and therefore not natural ones. In some places the schalstone seemed to be impregnated with cinnamon stone; for it had the colour of cinnamon stone with the foliated texture of schalstone.

The rock containing schalstone, which occurs in the Bannat, is likewise a triple compound, consisting of an aggregate of crystallized garnet, blue calcareous spar and schalstone. Hence it bears a resemblance to the Ceylon rock; for the cinnamon stone obviously belongs to the garnet family. The great difference between the two consists in the one containing quartz in place of the blue calcareous spar, which constitutes the ingredient in the other.

XVI. *Rocks in Lake Huron.*

In lake Huron in North America small islands occur, distinguished by the name of the flower-pot rocks, from their figure. The structure of these rocks, if it be correct, deserves the attention of mineralogists. They consist of three beds; the lowest bed is

lime-stone, over this lies a bed of clay-slate, and over this, constituting the surface of the whole, is a bed of granite. I do not know who the British officer was who sent drawings of these rocks to the Admiralty. It is impossible, therefore, to determine how far one can rely upon the testimony conveyed in these drawings.

XVII. *Rumford Prize.*

The council of the Royal Society has voted the Rumford prize to Dr. Wells for his Essay on Dew. We shall take this opportunity of pointing out an erratum in our last number. Instead of the Rumford medal being given to Dr. Brewster, as stated in p. 133 of the present volume, it should have been the Copleyan medal.

XVIII. *Caterpillars in Switzerland.*

A very singular phenomenon has lately taken place in Switzerland, at the distance of about nine miles from Lauzanne. The whole surface of the snow is covered with a species of caterpillar, different from any which are usually observed in that country. These animals appear dead; but when brought near a fire they soon recover animation.

XIX. *Composition of Alcohol and Ether.*

According to the calculations of Gay-Lussac, founded on the experiments of Saussure, alcohol is composed of

Olefiant gas .... 1 volume
Vapour of water .... 1 volume,

the whole condensed into half its bulk. While ether is composed of

Olefiant gas .... 2 volumes
Vapour of water .... 1 volume,

the whole condensed into one volume. He considers the specific gravity of olefiant gas as 0·978, and that of the vapour of water as 0·625. The specific gravity of the vapour of alcohol, according to his experiments is 1·613 and that of the vapour of ether 2·586. (See Annales de Chimie, xcv. 311.)

XX. *Sugar of Diabetic Urine.*

According to the recent experiments of Chevreul, the sugar of diabetic urine possesses all the characters of sugar of grapes. (See Annales de Chimie, xcv. 319.)

Article IX.

New Patents.

George Morton, Covent Garden, London; for a mode of attaching horses to waggons, and all other four-wheeled carriages. Nov. 14, 1815.

Joseph Baader, Doctor of Medicine, Knight, of the kingdom of Bavaria; for an improved plan of constructing rail roads, and carriages to be used on such improved rail roads, for the more easy, convenient, and expeditious, conveyance of all sorts of goods, wares, merchandize, persons, and all other articles usually or at any time removed in carriages of any construction whatever. Nov. 14, 1815.

James Dutton, jun. Hillsley, Gloucestershire, clothier; for certain improvements in fulling mills. Nov. 23, 1815.

Allan Taylor, Barking, Essex, Daniel Gallafent, sen. and Daniel Gallafent, jun. Braintree, Essex; for an engine for raising cold and hot water. Nov. 25, 1815.

George Young, Paul's Wharf, Thames-street, London; for a method of making a peculiar species of canvas, which may be used more advantageously for military and other purposes than the canvas now in use. Dec. 5, 1815.

John Malzi, Poland-street, London; for an instrument for the improvement of musical performance, which he calls a *metranome*, or musical time-keeper. Dec. 5, 1815.

Article X.

Scientific Books in hand, or in the Press.

Mr. Accum has in the Press a Third Edition of his Practical Treatise on Gas Light; exhibiting a summary description of the apparatus and machinery best calculated for illuminating streets, houses, and manufactories, with coal gas; with remarks on the utility, safety, and general nature, of this new branch of civil economy.

Baron Von Humboldt has just published the first half volume of his South American Plants. Its title is as follows: Nova Genera et Species Plantarum quas in Peregrinatione Orbis Novi colligerunt, descripserunt partim adumbraverunt Amat. Bonpland et Alex. de Humboldt, ex schedis autographis Amati Bonpland in Ordinem digessit Carolus Sigismund. Kuhn. Accedunt Tabulæ æris incisæ et Alex. de Humboldt Notationes ad Geographiam Plantarum spectantes. Paris, 1815.—We shall afterwards give an abstract of Humboldt's curious introduction respecting the geography of plants, which constitutes an interesting subject of contemplation for the philosopher.

Dr. Olinthus Gregory is about to publish Elements of Plane and Spherical Trigonometry, with their applications to heights and distances, projections of the sphere, dialling, astronomy, the solution of equations, and geodesic operations; intended for the use of mathematical seminaries, and of first year men at College.

The Rev. W. Bingley, Author of Animal Biography, has prepared for the press Useful Knowledge in the Mineral, Vegetable, and Animal Kingdoms; or a familiar account of the various productions of nature, which are chiefly employed for the use of man; in three volumes, 12mo. with numerous plates.

Article XI.

METEOROLOGICAL TABLE.

1816.	Wind.	Barometer. Max.	Min.	Med.	Thermometer. Max.	Min.	Med.	Hygr. at 9 a. m.	Rain.	
1st Mo.										
Jan. 21	S E	29·30	29·15	29·225	43	37	40·0	93	·16	☾
22	S E	29·42	29·30	29·360	44	36	40·0	93		
23	S E	29·42	29·16	29·290	43	37	40·0	81	—	
24	E	29·16	29·01	29·085	43	34	38·5	86	·17	
25	N E	29·17	29·01	29·090	39	34	36·5	96		
26	N E	29·52	29·17	29·345	40	34	37·0	76	3	
27	N	30·00	29·52	29·760	41	29	35·0	78	—	
28	N	30·30	30·00	30·150	36	29	32·5	84	—	
29	N E	30·38	30·30	30·340	33	23	28·0	85		●
30	S E	30·38	30·27	30·325	32	21	26·5	74		
31	S W	30·27	29·93	30·100	34	21	27·5	75		
2d Mo.										
Feb. 1	S E	29·93	29·64	29·785	35	19	27·0	85		
2	S W	29·64	29·42	29·530	40	26	33·0	100	·25	
3	S	29·47	29·43	29·450	48	37	42·5	98		
4	S W	29·40	29·37	29·385	45	33	39·0	100	—	
5	S	29·37	29·09	29·230	39	35	37·0	97	·15	
6	S E	29·09	28·90	28·995	38	31	34·5	77	—	☽
7	N E	29·31	28·90	29·105	31	15	23·0	76	1·38	
8	N	29·62	29·31	29·465	24	7	15·5	80		
9	E	29·68	29·62	29·650	20	−5	7·5	75		
10	S W	29·77	29·65	29·710	30	19	24·5	60		
11	N	30·25	29·75	30·000	37	18	27·5	56	—	
12	N	30·35	30·25	30·300	32	11	21·5	72		
13	Var.	30·31	30·24	30·275	36	22	29·0	88		○
14	W	30·35	30·31	30·330	39	25	32·0	99		
15	S W	30.35	29·96	30·155	44	32	38·0	75	—	
16	N E	29·82	29·73	29·775	47	33	40·0	56		
17	N W	30·04	29·82	29·930	38	26	32·0	54	—	
18	S W	29·88	29·77	29·825	41	27	34·0	80	—	
19	S W	29·96	29·88	29·920	45	37	41·0	70	7	
		30·38	28·90	29·696	48	−5	32·0	80	2·21	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

First Month.—21. A dripping day. 22. *Nimbi* grouped with other clouds: fine at mid-day. 23. Overcast. 24. Drizzling: rain in the night. 25. Overcast. 26. The same: a fog on the Thames appeared from hence to be a dense bank of cloud in the horizon: a little rain by night. 27. Fresh breeze: cloudy: p. m. a shower, with hail: night frosty. 28. Fresh breeze: drizzling rain: snow: fair, p. m. 29. Hoar frost: cloudy: fair. 30. Very white frost: misty horizon: sunshine after. 31. As yesterday, a. m.: at noon, hygr. 50°, the dust flies: wind S.W. p. m. with the usual sound for rain.

Second Month.—1. Hoar frost: fair. 2. Ice now about two inches thick: after hoar frost, a misty thaw: wet and windy evening. 3. Fair: moisture on the outside only of the windows. 4. Strong breeze at S.: misty: rain, followed by *Cumulus*, with *Cirrostratus*. 5. Very misty: the trees dripping. 6. Small and heavier rain by intervals: sleet at evening. 7. A gale from N. E., which came on last night, has brought a deep snow: snowy at intervals through the day. 8. A smart breeze, with clear sunshine: the roads sloppy at mid-day: some distant clouds in the horizon at sun-set. 9. A continued sun-shine produced not the least effect on the ice to-day: hygr. at three p. m. 47°: there was a mistiness perceptible, to a certain height, round the horizon: the wind a gentle breeze. 10. For remarks on this night, see the sequel. 11. Hygr. as yesterday nearly: sleet, snow, and rudiments of hail, in minute quantity. 15. After three days of clear sky (a little *Cirrostratus* excepted), an extremely misty air: different clouds followed, and a few drops by inosculation. 16. a. m. *Cirrostratus* in flocks: wind changed to S.W., then to N.W., and blew strong at night: hygr. receded to 46°. 17. Frost on the ground, from evaporation merely: the air by two thermometers not below 38°: the snow mostly gone; but a very thick ice remains on the water: *Cumuli* rose this morning, and passed to large spreading *Cumulostrati*. 18. Obscurity to the N. E: snow, p. m. which melted in the night.

RESULTS.

Winds Northerly and Easterly.

Barometer:	Greatest height	30·38 inches;
	Least	28·90 inches;
	Mean of the period	29·696 inches.
Thermometer:	Greatest height	48°
	Least	−5°
	Mean of the period	32°

Mean of the hygrometer at nine, a. m. 80°. Rain and melted snow, 2·21 inches.

A night on which Fahrenheit's thermometer remains for some hours below *Zero*, is, in this climate, a rare occurrence: probably not above five of them fall within a century; the last appearing to have been 19 years ago. It is observable that this extreme low temperature is not, as might be expected, peculiar to long continued frosts, but happens rather at an interval of one winter after such a season. Such was the frost of 1794-5, which lasted 44 days, one whole day's remission excepted, immediately before which the thermometer had descended to − 2; but in 1796-7, I find a temperature recorded of − 6, 5, with circumstances that indicate its having continued below *Zero* for some hours. Again, the character of the winter before last will be fresh in remembrance: the minimum of that season appears to have been not lower than 5°; and we have now a depression reaching

to — 5. I do not, however, lay much stress on this analogy, which is pointed out rather for the use of future observers.

I was prepared to expect the intense cold of the night of the 9th to 10th instant, by the circumstance of a temperature of 7°, (or probably 5°) on the night before, being followed by a clear sky, with the wind at E, and a maximum for the day of only 20°. Early in the evening, on trying the experiment of placing a wet finger on the iron-railing without, it was found to adhere immediately and strongly to the iron. I exposed several thermometers in different situations. At 8, p. m., a quicksilver thermometer, with the bulb supported a little above the snow, stood at *Zero*: at 11, p. m., a spirit thermometer in the same position indicated — 4; the former, which had a pretty large bulb, had not sunk below — 3. At half past seven, a. m., the 10th, a quicksilver and a spirit thermometer, hung over-night about eight feet above the ground, indicated respectively — 3, and were evidently rising. The minimum of — 5, which I have registered, was taken from a Six's thermometer, the freezing point of which is very correctly marked on the scale, placed a little above the snow. As the float of this thermometer had not room to move further, it may not have indicated the actual minimum of the air in that situation: but I have other evidence that, at the usual height from the ground, of my standard thermometer, the temperature was at no time below — 5. The exposure is North, and very open.

From eight, a. m. the thermometer continued to rise steadily: at noon a temperature of 25° was pleasant, by contrast, to the feeling, and it was easy to keep warm in walking without an upper coat. Even at *Zero*, however, the first impression of the air on the skin was not disagreeable, the dryness and stillness greatly tending to prevent that sudden abstraction of heat, which is felt in moist and quickly flowing air. Early in the afternoon the wind changed all at once to S. W.; some large *Cirri*, which had appeared all the day, passed to *Cirrocumulus* and *Cirrostratus*, with obscurity to the south. I now confidently expected *rain* (as had happened in former instances) but was deceived; and the thaw has taken place with a dry air for the most part, and with several interruptions by night.

During these two days the barometer, which had risen rapidly, fluctuated between 29·6 and 29·7 inches, and immediately after resumed its course, and rose at the same rate as before.

The mean temperature of this period is precisely 32°, and it is remarkable, that the mean temperature of each of three similar periods of frost, comprehended in the long winter of 1813-4, does not vary a degree from 32°; though preceded and followed by periods which respectively exhibit a mean of about 44°. On examination, I perceive that this analogy might be extended further.

The gale at N. E. with which this frost came in, brought with it abundance of snow, which loaded the trees to their tops, and weighed down the smaller shrubs to the ground. The peculiar clinging quality of some snows merits inquiry. It is in part the result of the needly crystallized texture, aided by a degree of moisture attending, which afterwards freezes in the mass; but as light volcanic ashes have been found likewise to possess this quality, and in a still higher degree, perhaps we ought to attribute something to the electric charge with which each of these light bodies arrives at the earth. The seasonable covering which snow affords to the vegetable kingdom is matter of common remark; but it is not so generally understood in how great a

degree the very circumstance of its production abates the first rigour of the cold. Just before this snow the air was extremely moist; the snow cleared it of an inch and a half, nearly, of water, and it has since indicated considerable dryness. Now it is quite probable that the *vapour* which afforded this water was found, by the supervening N. E. current, diffused in our local atmosphere, and by it decomposed. In this case the whole of the *latent or constituent heat* given out by the vapour in passing to the solid state, must have gone to raise the temperature of that current. Hence a considerable interval, of gradually increasing cold, before we experienced its extreme effects; during which, too, the earth got provided with its accustomed covering.

After a copious fall of snow an observer may find, in the scenery which it forms, some things on which to exercise his powers of reflection. The pensile drifts, which in a mountainous country are objects of just alarm, may be contemplated, here, to discover the principles of their construction, and the manner in which they rest on so narrow a base. When the sun shines clear, and the temperature is at the same time too low for it to produce any moisture, the level surface may be found sprinkled with small polished *plates of ice*, which refract the light in colours as varied and as brilliant as those of the drops of dew. At such times, there are also to be found on the borders of frozen pools, and on small bodies which happen to be fixed in the ice and project from the surface, groups of feathery crystals, of considerable size, and of an extremely curious and delicate structure. From the moment almost that snow alights on the ground, it begins to undergo certain changes, which commonly end in a more solid crystallization than that which it had originally. A notable proportion evaporates again, and this at temperatures far below the freezing point. On the night of the 10th instant I exposed 1000 grains of light snow, spread on a dish, (which had previously the temperature of the air) of about six inches diameter. In the first hour after dark it lost five grains; in the second, four grains; in the third it acquired a grain, the wind having changed, and the temperature, which had been falling from 25°, inclining to rise again. The hygrometer was at 50°, with a gentle breeze at east. In the course of the night the total loss was about 60 grains. This evaporation from snow may very well supply the water for forming those thin mists, which appear in intense frost: and the slight increase during a part of the time, in this experiment, may throw light on the formation of the secondary icy crystallizations above-mentioned. It appears that the air in a still frosty night becomes partially loaded, either with spiculæ of ice, or with particles of water, at a temperature below freezing, and ready to become solid the moment they find a support. Hence the rime on trees, which is found to accumulate chiefly on the windward side of the twigs and branches.

As to those more copious mists, of the modification *stratus*, which accompany the setting in of long frosts, I conceive them to originate in part from the yet unfrozen rivers, and other waters, near which they are most abundant; in part from the moisture of the earth itself: for it is contrary to experience to suppose, that the frozen state of the surface can prevent the ascent of vapour from the porous soil below: which will continue to emit it, until its temperature becomes, by the gradual penetration of the frost, nearly on a level with that of the cold air then constantly flowing over it, though too gently to disperse the cloud formed.

Tottenham, Second Month, 20, 1816. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

APRIL, 1816.

Article I.

Biographical Account of the late John Robison, LL. D. F. R. S. E. and Professor of Natural Philosophy in the University of Edinburgh. By John Playfair, F.R.S. L. & E. &c.

(*Concluded from* p. 183.)

In December, 1785, Mr. Robison was attacked by a severe disorder, which, with but few intervals of relaxation, continued to afflict him to the end of his life, and which, though borne with much resignation, and resisted with singular fortitude, could not but at length impair both the vigour and the continuity of his exertions. The disorder seemed to be situated between the urethra and the perineum. At times it was accompanied with the severest pain, and with violent spasms, which were easily excited. The disease, however, was only known by the pain produced; and never, by any visible or palpable symptom, gave information of its nature, as no change in the parts which were the seat of it could ever be observed. A complaint of this nature it is evident must have less chance of being removed than any other, and it accordingly baffled the art of the most skilful medical men both in Edinburgh and London.

Notwithstanding this state of suffering, his general health was not for a long time materially injured, nor the powers of his mind relaxed, so that he continued to prosecute study with vigour and steadiness. A malady which was both severe and chronical admitted of no palliative so good as the comfort of domestic society, which Mr. Robison happily enjoyed, having married soon after he settled in Edinburgh. The care and attention of Mrs. Robison, and the affectionate regards of his children, as they grew up, were blessings

to which, with all his habits of study and abstraction, he was ever perfectly alive.

This indisposition did not prevent him from engaging about this time in a very laborious undertaking. A work with the title of Encyclopædia Britannica, undertaken at Edinburgh several years before this period, was now undergoing a third edition, in which it was to advance from three to 18 volumes. Twelve of these had been already published, under the direction of the original editor, Mr. Colin Macfarquhar, when, on his death, the task of continuing the work was committed to the care of the Rev. Dr. Gleig, and about the same time Professor Robison became a contributor to it. He was the first contributor who was professedly and really a man of science, and from that time the Encyclopædia Britannica ceased to be a mere compilation. Dictionaries of Arts and Sciences in this island had hitherto been little else than compilations; and though in France the co-operation of some of the most profound and enlightened men of the age had produced a work of great merit and celebrity, with us compositions of the same class had been committed to the hands of very inferior artists. The accession of Professor Robison was an event of great importance in the history of the above publication.

It was in the year 1793 that he began to write in this book, and it was at the article Optics, with him a very favourite science, that his labours commenced. From that time he continued to enrich the Encyclopædia with a variety of valuable treatises, till its completion in 1801.

The general merit of the articles thus composed makes it difficult to point out particulars. Those in which theoretical and practical knowledge are combined are of distinguished merit; such are Seamanship, Telescope, Roof, Water-works, Resistance of Fluids, Running of Rivers. To these I must add the articles Electricity and Magnetism in the Supplement, where the theories of Æpinus are laid down with great clearness and precision, as well as with very considerable improvement. In ascertaining the law of the electric attraction, his experiments were ingenious, as well as original, and afforded an approximation to the result which the great skill and the excellent apparatus of Coulomb have since exactly ascertained. In the Supplement is also contained a very full account of the theory of Boscovich; a subject with which he was much delighted, and which he used to explain in his lectures with great spirit and elegance.

These articles, if collected, would form a quarto volume of more than a thousand pages. I am persuaded that when brought together, and arranged by themselves, they will make an acceptable present to the public; and I have the satisfaction to state that such a work is now preparing, under the direction of an editor whose remarks or corrections cannot but add greatly to its value. Notwithstanding the merit which the separate articles possess, they are not entirely

free from the faults incident to whatever is composed for a work already in the press. The condensation and arrangement, to which time is such an essential condition, even with men of the first talents, must be often wanting in such circumstances; and there are, accordingly, in the articles now referred to, a diffuseness, and sometimes a want of order, that may easily be corrected without injuring the authenticity of the work.

Though the Encyclopædia employed Professor Robison very much during the whole of the seven years that it continued, he nevertheless found leisure for some researches of a very different nature. At the period of which I now speak the French Revolution had arrested the attention, and excited the astonishment, of all Europe; and the satisfaction with which the first efforts of a nation to assert its liberties had been hailed from all quarters was, by the crimes and excesses which followed, quickly converted into grief and indignation. A body was put in motion sufficient to crush whole nations under its weight; none had the power or the skill to direct its course; what movements it might communicate to other bodies, how far it would go, or in what quarter, it seemed impossible to foretell. The amazement became general; no man was so abstracted from the pursuits of the world, or so insulated by peculiarities of habit and situation, as not to feel the effects of this powerful concussion. All fixed their eyes on the extraordinary spectacle which France exhibited; where, if time is to be measured by the succession of events, a year was magnified into an age; and when in a few months one might behold more old institutions destroyed, and more new ones projected or begun, than in all the ten centuries which had elapsed between Charlemagne and the last of his successors. In a word, where the ancient edifice, founded in the ages of barbarism with such apparent solidity, strengthened and adorned in the progress of civilization with so much skill and labour, was in one moment levelled with the dust. A general state of alarm and distrust was the effect of the convulsions which men saw every where around them; where the institutions held as sacred from their origin, or venerable from their antiquity, and essential to the order of society, were seen, not falling to pieces from natural decay, but blown up by the force of a sudden and unforeseen explosion. From such a condition of the world, jealousy and credulity could not fail to arise. When danger is all around, every thing is of course suspected; and when the ordinary connexion between causes and effects cannot be traced, men have no means of distinguishing between the probable and the improbable; so that their opinions are dictated by their prejudices, their impressions, and their fears. Such accordingly was the state into which men's minds were brought at this extraordinary crisis; and even in this country, removed as we were from the danger by so strong a barrier of causes, both moral and physical, the alarm was general and indiscriminate. The progress of knowledge was supposed by many to be the cause of the disorder; panegyrics on ignorance and prejudice were openly

pronounced; the serious and the gay joined in declaiming against reason and philosophy; and all seemed to forget that when reason and philosophy have erred, it is by themselves alone that their errors can be corrected.

The fears that had thus taken possession of men's minds were often artificially increased. It was supposed that the general safety depended on the general alarm; that the more the terror was extended, the more would the object of it be resisted; and hence doubtless many felt it their interest, and some considered it their duty, to magnify the danger to which the public was exposed.

It is evident that an inquiry into the causes of the French Revolution, undertaken at a moment of such agitation, was not likely to bear the review of times of calm and sober reflection. It was at this moment, however, and under the influence of such impressions, that Mr. Robison undertook to explain the causes of that revolution. He was deeply affected by the scenes that were passing before him. He possessed great sensibility, and his mind, peculiarly alive to immediate impressions, felt strongly the danger to which the social order of every nation seemed now to be exposed. The crimes which the name of Liberty had been employed to sanction, filled him with indignation, and the contempt of religion, affected by many of the leaders of the Revolution, wounded those sentiments of piety which he had uniformly cherished from his early youth.

In such circumstances, a mind accustomed to inquire into causes, as his had long been, could not abstain from the attempt to trace the sources of so extraordinary a succession of events. As to the circumstances which first led him, and led him I think so unhappily, to look for those sources in the institutions of Freemasonry, or in the combination of some German mystics, I have nothing satisfactory to offer. He was accustomed to refined and subtle speculations, and naturally entertained a partiality for theories that called into action the powers by which he was peculiarly distinguished.

In 1797 he published a book, entitled, Proofs of a Conspiracy against all the Religions and Governments of Europe. He supposes that this conspiracy originated in the Lodges of the Freemasons, but that it first assumed a regular form in the hands of certain philosophic fanatics distinguished in Germany by the name of *Illuminati*; that after the suppression of this society by the authority of Government, the spirit was kept alive by what was called the German Union; that its principles gradually infected most of the philosophers of France and Germany, and lastly broke forth with full force in the French Revolution.

The history of *Illuminatism*, as it is called, forms the principal part of the work; and on a subject involved in great mystery, where all the evidence came through the hands of friends or of enemies, it was exceedingly difficult for one living in a foreign country and a stranger to the public opinion, to obtain accurate information. Accordingly, the events related, and the characters described, as proofs of the conspiracy, are of so extraordinary a

nature, that it is difficult to persuade one's self, that the original documents from which Mr. Robison drew up his narrative were entitled to all the confidence which he reposed in them.

I do not mean to question the general fact, that there did exist in Germany a society having the vanity to assume the name just mentioned, and the presumption or the simplicity to believe that it could reform the world. In a land where the tendency to the romantic and the mysterious seems so general, that even philosophy and science have not escaped the infection, and in states where there is much that requires amendment, it is not wonderful if associations have been formed for redressing grievances, and reforming both religion and government. Some men, truly philanthropic, and others, merely profligate, may have joined in this combination; the former, very erroneously supposing, that the interests of truth and of mankind may be advanced by cabal and intrigue: and the latter, more wisely concluding, that these are engines well adapted to promote the dissemination of error, and the schemes of private aggrandisement. An ex-Jesuit may have been the author of this plan, and whether he belonged to the former or the latter class, may have chosen for the model of the new arrangement, those institutions which he knew from experience to be well adapted for exercising a strong but secret influence in the direction of human affairs.

In all this there is nothing incredible; but the same, I think, cannot be asserted when the particulars are examined in detail. It is extremely difficult, as has already been remarked, for a foreigner in such circumstances as Mr. Robison's to avoid delusion, or to determine between the different kinds of testimony of which he must make use. With me, who have no access to the original documents, and if I had, who have neither leisure nor inclination to examine them, an opinion can only be formed from the internal evidence, that is, from the nature of the facts, and the style in which they are recorded. The style of the works from which Mr. Robison composed his narrative is not such as to inspire confidence; for, wherever it is quoted, it is that of an angry and inflated invective. The facts themselves are altogether singular, arguing a depravity quite unexampled in all the votaries of *illumination*. From the perusal of the whole, it is impossible not to conclude, that the alarm excited by the French Revolution, had produced in Mr. Robison a degree of credulity which was not natural to him. The suspicion with which he seems to view every person on the continent, to whom the name of a philosopher can be applied, and the terms of reproach and contempt to which, whether as individuals or as bodies, they are always subjected, make it evident that the narrative is not impartial, and that the author was prepared, in certain cases, to admit the slightest presumption as clear and irrefragable evidence. When, indeed, he speaks of such obscure men as composed the greater part of the supposed conspirators, we have no direct means of determining in what degree he has been

misled. But when we see the same sort of suspicion and abuse directed against the best known and most justly celebrated characters of the age, we cannot but lament the prejudices which had taken possession of an understanding in other matters so acute and penetrating.

Among the men engaged in public affairs, of whom Europe boasted during the last century, there was perhaps none of a higher character than Turgot, who, to the abilities of a statesman added the views of a philosopher; was a man singularly patriotic and disinterested, distinguished by the virtues both of public and private life, and having, indeed, no fault but that of being too good for the times in which he lived. Yet Mr. Robison has charged this upright and humane minister with an exercise of power, which would argue the most extreme depravity. He states,* that there existed in Paris a combination under the direction of the Wits and Philosophers, who used to meet at the house of Baron D'Holbach, having for its object the dissection of the brains of living children, purchased from poor parents, in order to discover the principle of vitality. The police, he adds, interposed to put a stop to these bloody experiments, but the authors of them were protected by the credit of Turgot.

All this is asserted on the authority, it should seem, of some anonymous German publication. I will not enter on the refutation of a calumny with the fabrication of which Mr. Robison is not chargeable, though culpable without doubt, for having allowed his writings to become the vehicle of it. Truth and justice require this acknowledgment; and, in making it, I think that I am discharging a duty both to Mr. Robison and myself. It is a duty to Mr. Robison, in as much as a concession made by a friend is better than one extorted by an adversary; it is a duty to myself, because I should feel that I was doing wrong, were I even by silence to acquiesce in a representation which I believed to be so ill-founded and unjust.

The Proofs of the Conspiracy, notwithstanding these imperfections, or perhaps on account of them, were extremely popular, and carried the name of the author into places where his high attainments in science had never gained admission for it. In the course of two years the book underwent no less than four editions. It is a strong proof of the effect on the minds of men produced by the French Revolution, and of the degree in which it engrossed their thoughts, that the history of a few obscure enthusiasts in Bavaria or Wirtemberg, when it became associated with that Revolution, was read in Britain with so much avidity and attention.

The defects of the evidence were concealed by the prejudices and apprehensions which were then so general. The people of this country were disposed to believe every thing unfavourable to the French nation, but particularly to the philosophers. All

* Proofs of a Conspiracy, &c. 4th Edit. Note, p. 584.

might not be equally culpable, but to discriminate between them was not thought of much importance, and it was the simplest, if not the fairest way, to divide the demerit equally among the whole. The rhapsodies of Barruel had already prepared the public for such impartial decisions, and had held up every man of genius and talents, from Montesquieu to Condorcet, as objects of hatred and execration.

But whateve. opinion be formed of the facts related in the history of this conspiracy, it is certainly not in the visions of the German Illuminati, nor in the ceremonials of Freemasonry, that we are to seek for the causes of a Revolution, which has shaken the civilised world from its foundations, and left behind it so many marks, which ages will be required to efface. There is a certain proportionality between causes and their effects, which we must expect to meet with in the moral no less than in the natural world; in the operations of men as well as in the motions of inanimate bodies. Whenever a great mass of mankind is brought to act together, it must be in consequence of an impulse communicated to the whole, not in consequence of a force that can act only on a few. A hermit or a saint might have preached a crusade to the Holy Land, with all the eloquence which enthusiasm could inspire; but if a spirit of fanaticism and of chivalry had not pervaded every individual in that age, they would never have led out the armies of Europe to combat before the walls of Jerusalem. Neither could the influence of a small number of religious or philosophic fanatics, sensibly accelerate or retard those powerful causes which prepared from afar the destruction of the French monarchy. When opposed to these causes, such influence was annihilated; when co-operating with them, its effects were imperceptible. It was a force which could only follow those already in action; it was like "dashing with the oar to hasten the cataract," or, "waving with a fan to give swiftness to the wind."*

It is, however, much easier to say what were not, than what were, the causes of the French Revolution; and in dissenting from Professor Robison, I will only remark in general, that I believe the principal causes to be involved in this maxim, That a certain relation between the degree of knowledge diffused through a nation, and the degree of political liberty enjoyed by it, is necessary to the stability of its government. The knowledge and information of the French people exceeded the measure that is consistent with the entire want of political liberty. The first great exigency of Government, therefore, the first moment of a weak administration, could hardly fail to produce an attempt to obtain possession of those rights, which, though never enjoyed, can never be alienated. Such an occasion actually occurred, and the revolution which took place was entire and terrible. This also was to be expected; for there seems to be among political institutions, as among mechanical

* Ferguson's Essay on Civil Society, part iii. sect. 4.

contrivances, two kinds of equilibrium, which, though they appear very much alike in times of quiet, yet, in the moment of agitation and difficulty, are discovered to be very different from one another. The one is tottering and insecure, in so much that the smallest departure from the exact balance leads to its total subversion. The other is stable, so that even a violent concussion only excites some vibrations backward and forward, after which every thing settles in its own place. Those governments in which there is no political liberty, and where the people have no influence, are all unnavoidably in the first of these predicaments: those in which there is a broad basis of liberty, naturally belong to that in which the balance re-establishes itself. The same weight, that of the people, which in the first case tends to overset the balance, tends in the second to restore it: and hence, probably, the great difference between the result of the French Revolution, and of the revolutions which formerly took place in this country.

It will be happy for mankind, if they learn from these disasters, the great lessons which they seem so much calculated to enforce, and if while the people reflect on the danger of sudden innovation, their rulers consider, that it is only by a gradual reformation of abuses, and by extending, rather than abridging, the liberties of the people, that a remedy can be provided against similar convulsions.

But I return willingly from this digression, to those branches of knowledge, where, in describing what Mr. Robison has done, the language of truth and of praise will never be found at variance with one another.

In autumn 1799, this country had the misfortune to lose one of its brightest ornaments, Dr. Black, who had laid the foundation of the Pneumatic Chemistry, and discovered the principle of Latent Heat. The Doctor had published very little; and his discoveries were more numerous than his writings. His lectures, however, had drawn much attention; they presented the first philosophical views of chemical science; they were remarkable for their perspicuity and elegance, and this, joined to the simplicity and gracefulness of manner in which they were delivered, made them universally admired. It was now proposed to publish these lectures; but this required that they should be put into the hands of some one able to perform the part of an editor, and to prepare for the press the notes from which the Doctor used to read his lectures. The person naturally thought of was Mr. Robison, one of Dr. Black's oldest friends, and so well skilled in chemistry, that no one could be supposed to execute the work with more zeal or more intelligence. The task, however, was by no means easy. Dr. Black, with a very large share of talent and genius, with the most correct taste and soundest judgment, with no habits that could dissipate his mind, or withdraw it from the pursuits of science, was less ardent in research, and less stimulated by the love of fame, than might have been expected from such high endowments. A

state of health always delicate, and subject to be deranged by slight accidents, was probably the cause of this indifference. Hence the small number of his writings, and his sudden stop in that career of discovery on which he had entered with such brilliancy and success. Of much that he had done, the world had never heard any thing, but from verbal communication to his pupils; and on the subject of latent heat, no written document remained to ascertain to him the property of that great discovery. The only means of repairing this loss, and counteracting the injustice of the world, was the publication which Professor Robison now undertook with so much zeal, and executed with so much ability. Dr. Black had used to read his lectures from notes, and these often but very imperfect, and ranged in order by marks or signs only known to himself. The task of editing them was therefore difficult, and required a great deal both of time and labour, but was at last accomplished in a manner to give great satisfaction. The truth, however, is, that the time was past when this work would have met in the world with the reception which it deserved. Chemical theories had of late undergone great changes, and the language of the science was entirely altered. Dr. Black, on the subject of these changes, had corresponded with Lavoisier, and the mutual respect of two great men for one another was strongly marked in the letters which passed between them. The Doctor had acceded to the changes proposed by the French chemist, and had even adopted the new nomenclature; but his notes had not undergone the alterations which were necessary to introduce it throughout. It would now have been difficult to make those alterations; and Mr. Robison, who was not favourable to the new chemistry, did not conceive that by making them he was permanently serving the interest of his friend. He conceived, indeed, that there was unfairness in the means employed by Lavoisier, for bringing Dr. Black to adopt the new system of chemistry; and has thrown out some severe reflections on the conduct of the former, which appear to me to rest on a very slight foundation.

It was quite natural for a man, convinced, like Lavoisier, of the importance of the improvements which he had made in chemistry, to be desirous that they should be received by the most celebrated Professor of that time,—by the very man, too, whose discoveries had opened the way to those improvements. His letters to Dr. Black, contain expressions of respect and esteem, which, I confess, appear to me perfectly natural, and without any thing like exaggeration or deceit. Indeed it is not probable that M. Lavoisier, even if he could himself have submitted to flatter or cajole, could conceive that any good effect was to arise from doing so, or that there was any other way of inducing a grave, cautious, and profound philosopher, to adopt a certain system of opinions, but by convincing him of their truth. He had, with those who knew him, the character of a sincere man, very remote from any thing like art or affectation. We must therefore ascribe the view

which Mr. Robison took of this matter, to the same system of prejudices on which we have had already occasion to animadvert. Such, indeed, was the force of those prejudices, that he considered the chemical nomenclature, the new system of measures, and the new calendar, as all three equally the contrivances of men, not so much interested for science, as for the superiority of their own nation. Now, whatever be said of the calendar, the project of uniform weights and measures is admitted to be an admirably contrived system, which Britain is now following at a great distance; and the new nomenclature of chemistry to be a real scientific improvement, adopted all over Europe. Many of the radical words may depend on false theories, and may of course require to be changed; but though the *matter* pass away, the *form* will remain; the words of the language may perish, but the mould in which the language was cast will never be destroyed.* The Lectures appeared in 1803.

The last of Mr. Robison's works was one which he had long projected, though he now set about the completion and arrangement of it for the first time. It was entitled, Elements of Mechanical Philosophy, being the Substance of a Course of Lectures on that Science. "Mechanical philosophy" was with him a favourite expression; it was understood as synonymous with natural philosophy, and included the same branches. The first volume, the only one he lived to finish, included dynamics and astronomy, and was published in 1804. It is a work of great merit, and is accessible to those who have no more than an elementary knowledge of the mathematics. The short view of the phenomena prefixed to the physical astronomy is executed in a masterly manner. The same may be said, and perhaps even with more truth, of the physical astronomy itself; for there are very few of the elementary treatises on that branch of science which can be compared with it, either for the facility of the demonstration, or the comprehensiveness of the plan. The first part is meant to be popular and historical, and is so at the same time that it is philosophical and precise. The work is indeed highly estimable, and is entitled to much more success in the world than it has actually had.

We have already taken notice of Mr. Robison's illness, with which he had been now afflicted for the long period of 19 years. His sufferings, though not equal, had been often extremely severe. They had occasionally rendered him unable to discharge his duty in the college, and of late his friend, the Rev. Dr. Thomas Macknight, had, with great kindness and ability, frequently supplied

* The high opinion which Mr. Robison elsewhere expresses of Lavoisier, is very remarkable. In his Astronomy, published a year after the Lectures, in stating Hook's anticipation of the principles of gravitation, he concludes thus: "It is worthy of remark, that in this clear and candid and modest exposition of a rational theory, Hook anticipated the discoveries of Newton, as he anticipated with equal distinctness and precision, the discoveries of Lavoisier, a philosopher inferior perhaps only to Newton."—(*Elements of Mechanical Philosophy*, p. 285.)

his place. Against such a continuance of ill health, with so little hopes of recovery as could be entertained for a long time past, hardly any mind could be expected to remain in full possession of activity and vigour. This is the more difficult, as the valuable medicine which alone in such cases can assuage pain, contributes itself at length to weaken the mind, and to destroy its energy. The combat which Mr. Robison had maintained against these complicated evils, had indeed been wonderfully vigorous and successful, and the last of his works is quite worthy of his days of most perfect health and enjoyment.

The body could not resist so well as the mind. In the end of January 1805, he was suddenly seized with a severe illness, which put an end to his life in the course of 48 hours. There was a general disturbance of the system, which, without having the character of any defined disease, exhibited those symptoms of universal disorder which denote a breaking up of the constitution, and never fail to terminate fatally.

On reviewing the whole of his character, and the circumstances of his life, it is impossible not to see in him a man of extraordinary powers, who had enjoyed great opportunities for improvement, and had never failed to turn them to the best account. He possessed many accomplishments rarely to be met with in a scholar, or a man of science. He had great skill and taste in music, and was a performer on several instruments. He was an excellent draughtsman, and could make his pencil a valuable instrument either of record or invention. When a young man, he was gay, convivial, and facetious, and his *vers de société* flowed, I have been told, easily and with great effect. His appearance and manner were in a high degree favourable and imposing; his figure handsome, and his face expressive of talent, thought, gentleness, and good temper. When I had first the pleasure to become acquainted with him, the youthful turn of his countenance and manners was beginning to give place to the grave and serious cast, which he early assumed; and certainly I have never met with any one whose appearance and conversation were more impressive than his were at that period.

Indeed his powers of conversation were very extraordinary, and when exerted, never failed of producing a great effect. An extensive and accurate information of particular facts, and a facility of combining them into general and original views, were united in a degree of which I am persuaded there have been few examples. Accordingly, he would go over the most difficult subjects, and bring out the most profound remarks, with an ease and readiness which was quite singular. The depth of his observations seemed to cost him nothing; and when he said any thing particularly striking, you never could discover any appearance of the self-satisfaction so common on such occasions. He was disposed to pass quite readily from one subject to another; the transition was a matter of course, and he had perfectly, and apparently without seeking after it, that light and easy turn of conversation, even on scientific and profound

subjects, in which we of this island are charged by our neighbours with being so extremely deficient.

The same facility, and the same general tone, was to be seen in his lectures and his writings. He composed with singular facility and correctness, but was sometimes, when he had leisure to be so, very fastidious about his own compositions.

In the intercourse of life, he was benevolent, disinterested, and friendly, and of sincere and unaffected piety. In his interpretation of the conduct of others, he was fair and liberal, while his mind retained its natural tone, and had not yielded to the alarms of the French Revolution, and to the bias which it produced.

His range in science was most extensive; he was familiar with the whole circle of the accurate sciences, and there was no part of them on which, if you heard him speak or lecture, you would not have pronounced it to be his *fort*, or a subject which he had studied with more than ordinary attention. Indeed, the rapidity with which his understanding went to work, and the extent of ground he seemed to have got over, while others were only preparing to enter on it, were the great features of his intellectual character. In these he has rarely been exceeded. With such an assemblage of talents, with a mind so happily formed for science, one might have expected to find in his writings more of original investigation, more works of discovery and invention. I must remark, however, that from the turn his speculations and compositions took, or rather received from circumstances, we are apt to overlook what is new and original in a great part of them. An article in a dictionary of science must contain a system, and what is new becomes of course so mixed up with the old and the known, that it is not easily distinguished. Many of Mr. Robison's articles in the Encyclopædia Britannica are full of new and original views, which will only strike those who study them particularly, and have studied them in other books. In *Seamanship*, for example, there are many such remarks; the fruit of that knowledge of principle which he combined with so much experience and observation. *Carpentry*, *Roof*, and many more, afford examples of the same kind. The publication now under the management of Dr. Brewster, will place his scientific character higher than it has ever been with any but those who were personally acquainted with him. With them, nothing can add to the esteem which they felt for his talents and worth, or to the respect in which they now hold his memory.

Article II.

Account of an Accident which happened in a Coal-Mine at Liege in 1812. By Thomas Thomson, M.D. F.R.S.

In the preceding volumes of the *Annals of Philosophy* a variety of dismal accidents has been detailed, which occurred in the coal-

mines of the county of Durham. I shall here relate an accident similar to one of those which occurred four years ago at Liege, because it was attended with circumstances which deserve to be known. The account was published in the French newspapers at the time when it happened; but the following statement is taken from the Voyage dans la Belgique, by M. Paquet-Syphorien, published at Paris in 1813, who had the details from M. Goffin himself.

Just without the gate of the city of Liege, towards Brussels, several coal-mines are wrought. There are three perpendicular shafts at no great distance from each other, called Bure* Triquenotte, Bure de Beaujonc, and Bure Mamonster. The first two communicate with each other below ground; but there was no communication between the last two. In these mines, which are about 120 fathoms deep, the water is directed to a particular part of the mine, where it is confined by a wooden frame, called *serrement*, from which it is raised to the surface by means of forcing pumps.

On the 28th of February, 1812, about eleven o'clock in the forenoon, the mine connected with the shaft Beaujonc was suddenly inundated by the rupture of the serrement of the shaft Triquenotte, situated at the distance of 459 feet from the mine Beaujonc. At that time 127 workmen were in the mine, 35 of whom made their escape at the moment when the inundation took place. The overseer, M. Goffin, with his son, was at the bottom of the shaft, and might have easily made his escape; but he formed the resolution of saving his miners, or of perishing with them.

He gave orders to Nicholas Bertrand, Mathieu Labeye, and Clavier, three miners who were sharers in his generous resolution, to go and warn their companions, and to direct them to the part of the mine nearest the shaft Mamonster. Meanwhile he assisted all the workmen who had collected at the foot of the shaft to make their escape. The danger became at last so great that these men did not hesitate to tear the boys by force from the ropes of the basket, to which they had fixed themselves, and to take their places. But M. Goffin took up the poor boys, and carried them along with him. By the time that 35 of the miners had made their escape, the waters had risen to such a height as to cut off all communication with the shaft.

M. Goffin collected all the miners in that part of the mine which he considered as nearest to the shaft Mamonster, and, assisted by some of the stoutest of them, he undertook to open a passage into one of the galleries connected with that shaft. They were in possession of a few candles; but had no food. Though only two workmen could be employed at a time, they had already penetrated 23 feet, when a violent explosion of inflammable air took place, and informed them that they had been penetrating, not into the galleries

* *Bure* is the miner's term for a shaft.

of Mamonster, but into the old workings of Martin Wery. Some of the workmen proposed to continue the work in the same direction; but M. Goffin prevented them, saying, at the same time, "When we have no longer any hopes left, I shall conduct you to this place, and then all will soon be over."

At first they refused unanimously to obey him, and gave themselves up to despair. The young boys threw themselves on their knees to request a blessing from their parents, while the old men uttered dismal complaints, and lamented over the future lot of their wives and children. Goffin gradually inspired them with some courage, and prevailed upon them to proceed to the fifth gallery, which he judged to promise the shortest communication with the galleries of Mamonster. When they came to this place, they heard a distant noise, which rekindled some hope in their hearts, as they supposed it to proceed from their fellow miners opening a passage to them from the galleries of Mamonster. But they were by this time so exhausted by their former labours, and by the want of food, that all the exertions of Goffin were scarcely sufficient to inspire them with any activity. Three times they threw down their tools in absolute despair; but sometimes by entreaties, and sometimes by threats, he always prevailed upon them to resume their pickaxes, and recommence their work. They had dug a gallery 36 feet in length, though by the second day their candles had gone out, from the badness of the air, and they were left in total darkness.

For the first two or three days they suffered dreadfully from hunger. Some devoured the candles which they had contrived to conceal, and found in their own urine a drink, which they preferred to the putrid water of the mine. Others (Bertrand in particular) reckoned upon the speedy death of some of their companions as a means of furnishing them with food. Fortunately nature dissipated for a time at least these scenes of horror, by giving them the refreshment of a sound sleep.

Meanwhile every thing had been done without the mine for the deliverance of the unfortunate workmen thus buried alive, by the sagacious and vigorous orders of the Prefect. The shaft Mamonster presented the only means of letting them escape; but they had no exact plan of the workings, and knew not, therefore, through how much ground they had to penetrate in order to reach the galleries of Beaujonc. Above a hundred horses were kept constantly employed in pumping out the water, in order to prevent it from filling all the galleries. Twenty fresh men descended every four hours by the shaft Mamonster, in order to relieve the workmen who were pushing a gallery towards Beaujonc, without any loss of time. The engineer, M. Migneron, had ascertained with much sagacity the true geometrical point from which the gallery must commence, in order to reach the unfortunate sufferers. For greater certainty, they employed blasting, till they were certain that they had been heard by the sufferers; then their zeal was redoubled, and the exertions made were incredible.

The noise made by the buried miners while endeavouring to penetrate to Mamonster became gradually louder and louder; and on the fifth day they were able to communicate with Goffin and his unfortunate party. They were informed they were 74 in number, that none of them had perished, but that they were distressed by a dreadful heat, though sunk to the middle of their bodies in water. From that time they wrought without light in the mine of Mamonster, to prevent the inflammation of the air.

A communication was opened on the 3d of March, at seven in the evening, and every precaution was taken to prevent any fatal effects from the air, or from fire. After penetrating through a space of 511½ feet, a kind of detonation took place, from the escape of the condensed air. The unfortunate miners were then taken out, and every possible care was taken to prevent any injury from too sudden an exposure to the air and light. They were fed with a little wine and broth, then wrapped up in flannels, and laid for some time on straw in the mine itself before they were brought above ground. M. Goffin, though the most exhausted of all, came out last, with the engineer, M. Migneron, and young Mathieu Goffin. This extraordinary boy had given constant proofs of the greatest coolness and courage. On seeing his mother, he called out to her jocosely, *What, mother, are you not married again yet?** When the miners were in despair, and bursting into tears, he called out to them, "Come along, you behave like children, follow the orders of my father. We must work, and show those that survive us that we retained our courage to the last moment of our lives."

Article III.

On the Re-union of Parts accidentally separated from the Living Body. By Thomas Thomson, M.D. F.R.S.

I have of late received various letters containing queries on this subject. I conceive that the shortest and most satisfactory mode of answering them will be to give an historical detail of all the facts on the subject, as far as I am acquainted with them.

It is supposed that the first person who discovered that separated parts might be again made to adhere as before to the living body was Gaspard Tagliacozzi, a surgeon of Bologna, and Professor of Surgery in that city, who died in 1553. He published two treatises on the subject, with the following titles. De Curtorum Chirurgia per Insitionem, libri duo.— Chirurgia nova de Narium, Aurium, Labiorumque Defectu, per Insitionem Cutis ex Humeris arte hactenus ignota sarciendo. The first of these tracts is to be

* Eh bien, mère, n' etes-vous pas encore remariée?

found in the Bibliotheca Chirurgica of Mangetus, vol. i. p. 377. I have never seen the second. The first is written in so very tedious a style, and is so full of repetitions, that I never could find patience to read it through. His method appears to have been to supply artificial noses to those persons who had lost that organ (a very common case in those days) by a quantity of skin cut from the fore-arm. His practice does not seem to have had any followers, and indeed was soon thrown into ridicule. The verses of Butler upon the subject are well known, I presume, to all my readers.

Garengeot, about the middle of the last century, describes a case scarcely less extraordinary than those of Tagliacozzi. A person in a quarrel had his nose bit off. It was thrown upon the ground, and allowed to remain till it became quite cold. In this state it was washed, and applied again to the face. It adhered very speedily, and became as entire as at first. This case, which is minutely described by Garengeot, was at first neglected by his contemporaries, and afterwards thrown into ridicule.

The experiments of John Hunter upon fowls were well calculated to draw the attention of medical men. He made the spur of the cock grow upon his comb, and made the testicles of cocks adhere and live upon different parts of the hen. His account of transplanting teeth is equally striking. If a tooth be pulled out of the head of one person, and fixed in the jaw of another, it takes root, and fixes, and becomes liable to pain and disease precisely like other living teeth. Similar experiments were made, likewise, I believe, by Duhamel.

Two facts somewhat similar to the one stated by Garengeot are given by Dr. Balfour in the Edinburgh Medical and Surgical Journal for October, 1814. 1. About the year 1803 Mr. Gordon, at present a surgeon in India, paid a visit to Dr. Balfour, and when he went away pulled too the door after him with force. Unfortunately a little boy, aged about four years and a half, the son of Dr. Balfour, who was playing about, had his hand caught within the door, and immediately set up a dreadful scream. Mr. Gordon returned with the boy in his arms, who appeared in a state of torture. Three of his fingers had been cut off so completely that their points were only attached to the rest of the fingers by a small bit of skin. The fore-finger was divided through the middle of the nail, the middle finger a little below the nail, and the ring finger at the root of the nail itself. The amputation was as neat as if it had been made with a sharp instrument; though at the same time the fingers were terribly bruised. Dr. Balfour, shocked that his son should be maimed for life, put the extremities of the fingers in their places, and with the assistance of Mr. Gordon dressed them, though without any hopes of their adhering. But six days after, on taking off the bandage, to the inexpressible joy of Mr. Gordon and himself, the adhesion was found complete. The skin and the nails of the three fingers separated, but were speedily renewed, and the cure was so perfect that it required a very minute inspection to find any

difference between the one hand and the other. There was merely a slight scar at the bottom of the nail of the fourth finger.

2. On the 10th of June, 1814, at eleven o'clock in the forenoon, two men called upon Dr. Balfour; one of whom, George Peddie, carpenter, had his left hand tied up in a handkerchief, from which blood dropped. On examination, he found the index mutilated. About one-half of it had been cut off. On asking him what had become of it, he said that he had not looked for it, but supposed it would be found in the place where the accident had happened. Dr. Balfour instantly sent his companion, Thomas Robertson, to find it out, and bring it to him. The amputation had been made from the upper part of the second phalanx on the side of the thumb to the third phalanx on the opposite side. It had been made by an axe, and was very neat. Thomas Robertson returned in about five minutes with the piece of the finger, which was white and cold, and resembled, both in feeling and appearance, a piece of candle. It was $1\frac{1}{2}$ inch long on one side, and one inch on the other. Cold water was poured upon the two wounded surfaces to clean them. They were then applied to each other as exactly as possible; and Dr. Balfour assured the man that they would re-unite. He listened with distrust; on which Dr. Balfour endeavoured to convince him that at all events the experiment might be tried without any inconvenience, and told him that, unless pain or putrefaction came on, the bandage must not be untied for a week at least. He was advised to suspend his arm in a sling, and to abstain for some days from all work. He promised, at last, to obey these directions. Next day he returned, and stated that he had experienced no pain or inconvenience, but that the wound had continued to bleed a little. Dr. Balfour assured him that this was of no consequence, and engaged him to return every day. But he did not return again; nor did Dr. Balfour hear any thing of him till the 2d of July, when a person called upon Dr. B., and gave him the following account. Two days after the wound had been dressed, George Peddie, yielding to the ridicule of his companions, who laughed at him for giving credit to what the Doctor had told him, went to consult another practitioner. This Gentleman at first pointed out to him the impropriety of his conduct in going to any other but the person who had dressed him first; but when the patient insisted on his strong repugnance to carry a piece of dead flesh at the end of his finger, and requested him to take off the bandage, the surgeon at last consented. Fortunately the re-union had commenced, which engaged the surgeon to replace the bandage, and to persevere in the treatment which Dr. Balfour had begun. On the 4th of July Dr. Balfour visited his patient. The union was complete, and the finger had recovered its heat and sensibility. The skin and nail afterwards came off, but there was not the least doubt that they would be speedily renewed. Dr. Balfour terminates his relation with the affidavits of George Peddie, Thomas Robertson, and Dr. Reid (who was present when the finger was first dressed), before Mr. Duncan Cowan, Justice of

Peace, in order to remove all doubts respecting the authenticity of this extraordinary cure.

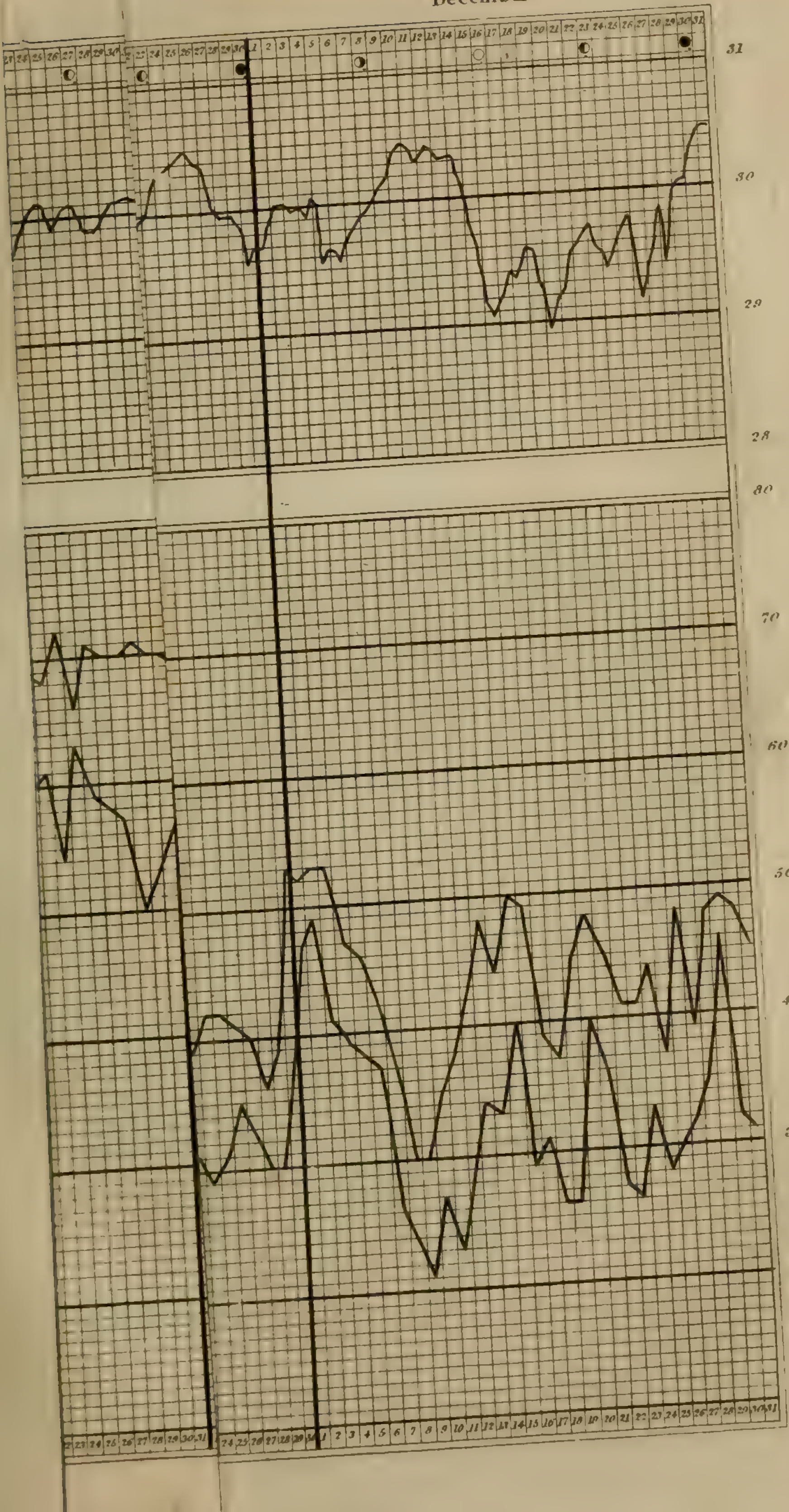
Mr. William Henry Bailey, surgeon at Thetford, in the county of Norfolk, having read the above history of George Peddie, as related by Dr. Balfour, an opportunity soon came in his way of repeating it with equal success. A labourer happening to separate the first phalanx of his middle finger from the rest, applied an hour and a half after to Mr. Bailey to get the wound dressed. Mr. Bailey washed the wound and the amputated portion, replaced it, and fixed it in its place by means of an adhesive plaster and a very simple bandage. A week after he found the re-union complete. A pulsation was felt distinctly at the end of the finger, and its colour was natural. The patient complained merely of some numbness in it. The nail, which had received a violent contusion, fell off 15 days after. The union was complete in five weeks. The amputated finger is as strong and sensible as the others; but the first phalanx has lost the power of flexion.—*Edinburgh Medical and Surgical Journal, July*, 1815.

A Lady at Lausanne having read the account of George Peddie, which was published in the Bibliotheque Britannique for May, 1815, vol. lix. p. 42, had likewise an opportunity of putting its accuracy to the test of experiment. Her cook maid, with a large knife, had accidentally cut off a large piece of flesh from her thumb, which was laid bare to the bone. The Lady replaced the piece which had been cut out, and covered the thumb with a large quantity of raw sugar, which with the blood formed a kind of paste. In 24 hours the re-union was complete, and the woman was able to go about her occupations as usual.—*Bibliotheque Britannique, vol.* lx. *p.* 100, *September*, 1815.

I must also state a remarkable cure performed by M. Percy, as he relates it under the article *Entes Animales*, in the 12th volume of the Dictionaire des Sciences Medicales, while commenting on the case of George Peddie. At the battle of Arlon, a soldier, while his right arm was elevated, and ready to strike, received such a dreadful blow with a sabre that his arm was entirely cut off, except a portion of skin under which, fortunately, the artery and nerve had been preserved entire. "I did not despair," says the author, "to save the arm, and I replaced it with so much care and precaution that I succeeded." The cure took up eight months; but at last the bone, the muscles, and the skin cicatrized completely. The limb remained long feeble and benumbed, and the fore-arm and hand did not recover their ordinary size. The last two fingers were paralytic. But Thiery, on his return to the department of Vosges, was able to occupy himself in the affairs of husbandry, which had been his first employment.

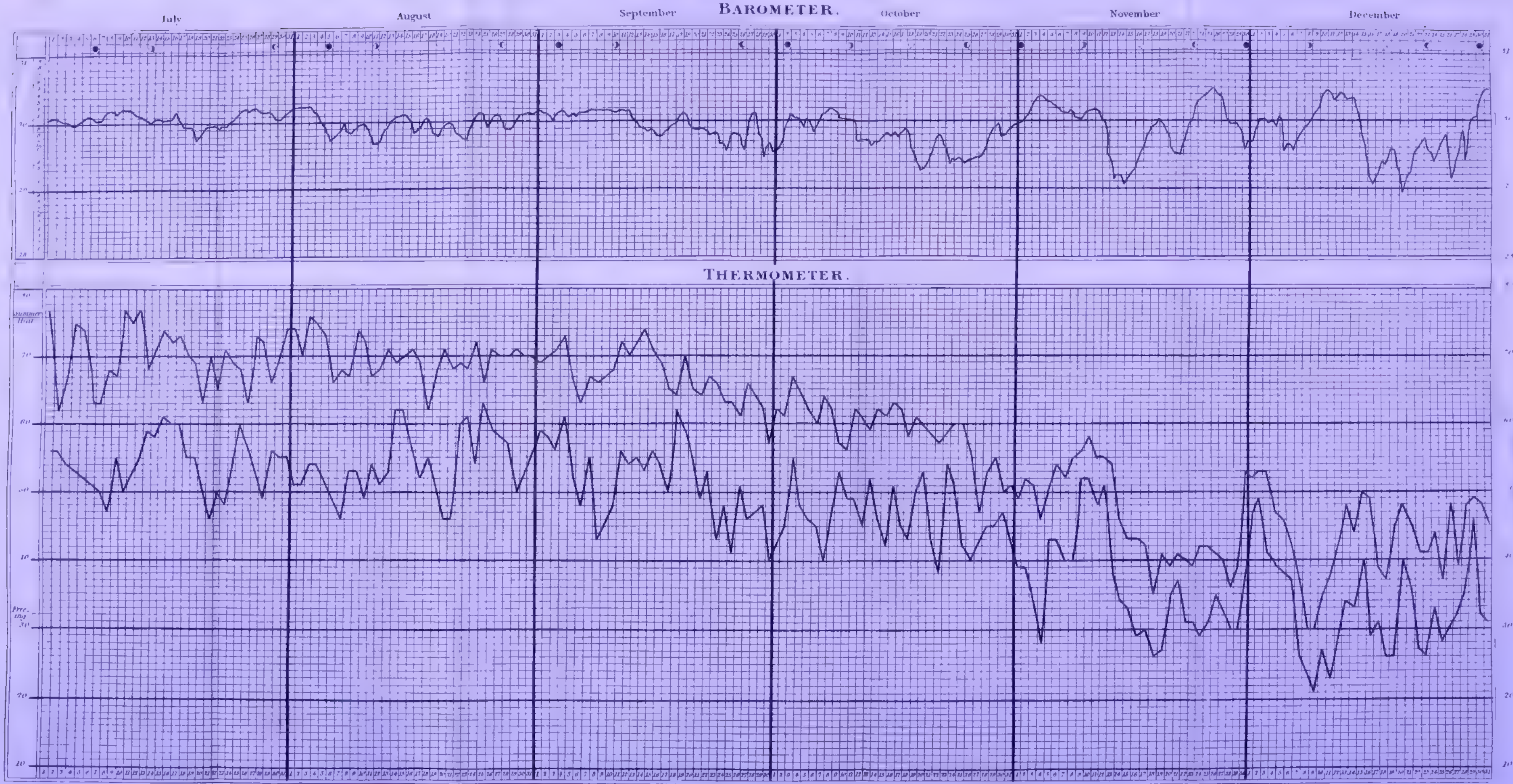
In the year 1804 a curious work on this subject was published at Milan by Mr. Joseph Baronio, entitled Degli Innesti Animali, which I know only by the account of it given by the editors of the Bibliotheque Britannique, vol. lix. p. 59. It relates a number of

December



Scale of the BAROMETER and THERMOMETER at Plymouth, July to December 1815.

Place of Observation 112 feet above the level of the sea.



Engraved for Dr. Thomson's Annals — for Baldwin Cradock & Joy Paternoster Row April 1 1816

experiments made upon sheep. The wool on the back of the animal was shaved off, and a piece of skin three inches long and two inches broad was cut out on each side of the spine near the tail. The skin from the left side was applied to the wound on the right, and *vice versâ.* The pieces were then fixed on with bandages. On the eighth day the bandages were removed, and the portions were found adhering. This experiment was repeated several times. Larger portions were cut out, and they were allowed to remain for an hour upon a table before they were applied again; yet they adhered as well as in the first case. A similar experiment made upon the neck of a cow did not succeed, in consequence of the impossibility of preventing the motion of the neck.

Such are the facts respecting this curious and important subject, as far as I am acquainted with them. They seem amply sufficient to remove the scepticism of the most obstinate unbeliever; and I have no doubt that the practice will speedily become quite common. Dr. Balfour seems entitled to the whole merit of having called the attention of practitioners to this great process of nature, and to have demonstrated its practicability in the most satisfactory manner.

Article IV.

Register of the Weather in Plymouth for the last Six Months of 1815. By James Fox, jun. Esq. With a Plate. [XLVI.]

JULY.

Date.	Wind.	Rain.	Observations.
1815.			
July 1	E N E		Fair.
2	E N E		High wind; cloudy and fair.
3	E N E		Ditto, ditto.
4	E to S E		Fair.
5	S E to S S E		Ditto.
6	W N W		Ditto.
7	W N W to N N W		Cloudy and fair.
8	W N W		Ditto, ditto.
9	W N W		Ditto; fair at night.
10	Var.		Ditto, ditto.
11	Var.		Fair.
12	Var.		Cloudy and fair.
13	S to W	0·21	Cloudy morn; rain afternoon.
14	S	0·05	Misty morn: cloudy afternoon.
15	S W to W		Ditto, ditto.
16	W N W		Cloudy day.
17	W		Ditto morn; cloudy and fair afternoon.
18	W N W	0·02	A shower, morn; cloudy day.
29	N W	0·08	Showers, morn; cloudy and fair aftern.
20	N W		High wind; cloudy and fair.
21	N W to S W	0·04	Cloudy and fair morn: showers aftern.
22	Var.		Cloudy and fair.

Date.	Wind.	Rain.	Observations.
1815.			
July 23	Var.	0·20	Thunder and showers, morn; cloudy and fair afternoon.
24	Var.		A light shower, morn; ditto, ditto, aftern.
25	N W		Cloudy and fair.
26	N W		Ditto, ditto.
27	Var.		Fog, morn; fair day.
28	S to S E		Fair day; cloudy at night.
29	S	0·62	Heavy rain.
30	N W to N N W	0·29	Ditto morn; fair afternoon.
31	Var.		Cloudy and fair.
		1·51	Inch rain.

			Wind.
Barometer:	Greatest height	30·21 inches	N W
	Lowest	29·75	N W
	Mean	30·067	
Thermometer:	Greatest height................	77°	E N E
	Lowest	46	Var.
	Mean	61·983	

AUGUST.

Aug. 1	Var.		Fair.
2	N W		Ditto.
3	Var.		Ditto.
4	Var.		Ditto.
5	W N W	0·13	Fog, morn; showers, afternoon; cloudy and fair night.
6	N W	0·37	High wind, and showers.
7	N W to S S E	0·15	Cloudy and fair morn; showers at night.
8	E N E	0·24	Showers early, morn; cloudy and fair day.
9	N W		Fair.
10	S W to W N W	0·25	High wind and showers, morn; cloudy and fair afternoon.
11	W N W		Ditto, ditto; stormy at night.
12	W N W	0·14	Ditto, ditto; squally.
13	W N W		Cloudy and fair.
14	W N W to S W	0·10	Misty day: high wind at night.
15	W S W		Misty.
16	S to W N W		Cloudy and fair morn; fair afternoon.
17	W N W	0·30	High wind; thick weather.
18	S S W		Ditto, ditto, and showers.
19	N W		Cloudy and fair morn; fair afternoon.
20	N W		Ditto, ditto.
21	E		Ditto; cloudy afternoon.
22	S S W to W	0·69	High wind, and heavy rain.
23	S W to W		Ditto, ditto.
24	S	0·11	Thick weather.
25	S S E		Ditto; high wind.
26	S S W		Cloudy and fair morn; fair afternoon.
27	S to S E	0·05	Misty.
28	S to W N W		Ditto.
29	W N W		Fair.
30	S S W to N W		Cloudy morn; fair afternoon.
31	W to W N W		Misty morn; cloudy and fair afternoon.
		2·53	Inches rain.

			Wind.
Barometer:	Greatest height	30·23 inches	Var.
	Lowest	29·67	W N W
	Mean	29·957	
Thermometer:	Greatest height	76°	Var.
	Lowest	46	N W
	Mean	61·741	

SEPTEMBER.

Date.	Wind.	Rain.	Observations.
1815.			
Sept. 1	Var.		Misty morn; fair day.
2	S to N W	0·12	Cloudy and fair morn; misty at night.
3	N W to W N W		Fair day; misty at night.
4	W N W		High wind; cloudy and fair.
5	N W to N N W		Fair day; ditto, ditto, night.
6	N N E to N N W		Cloudy and fair.
7	Var.		Fair.
8	E		Ditto.
9	E		Ditto.
10	Var.		Ditto; cloudy at night.
11	N W		Cloudy and fair.
12	E N E		Fair.
13	E		Ditto.
14	E N E		Ditto; cloudy at night.
15	S to E N E	0·15	Misty morn; showers, ditto.
16	S to N W		Showers early, morn; fair day.
17	S to S S E	0·97	A dirty day.
18	S		Heavy rain.
19	E N E		Cloudy and fair.
20	E		High wind; ditto.
21	E N E		Ditto, ditto.
22	S W to N W		Cloudy and fair.
23	W N W to S	0·36	Fair day; showers at eve.
24	S to W N W		Cloudy, and showers.
25	S W to S		Cloudy and fair; a gale of wind at night.
26	S	0·29	Stormy, and showers.
27	W to N	0·13	High wind, and showers.
28	E	0·44	Cloudy and fair, heavy showers, eve.
29	S E to W N W		Showers, morn; cloudy day; fair at night.
30	N E to S, W N W	0·23	Showers, and a gale at south.
		2·69	Inches rain.

			Wind.
Barometer:	Greatest height	30·17 inches	E
	Lowest	29·44	W N W
	Mean	29·948	
Thermometer:	Greatest height	74°	E N E
	Lowest	40	Var.
	Mean	58·850	

OCTOBER.

Date.	Wind.	Rain.	Observations.
1815.			
Oct. 1	W to N W	0·15	Thunder, and showers.
2	W N W to N N W		Cloudy and fair.
3	S		Ditto.
4	N W	0·74	Showers, morn; fair afternoon.
5	S		Cloudy morn; rain at night.
6	N W		Cloudy and fair.
7	N N W to E		Ditto.
8	E		Ditto; high wind.
9	E		Cloudy; ditto.
10	E		Ditto, ditto.
11	N E	0·04	Ditto; a shower.
12	N E to S S E	0·61	Ditto.
13	S E		High wind, and heavy rain.
14	S to W N W	0·08	Showers.
15	S	0·05	Thick weather.
16	S to W N W	0·08	Cloudy; showers at night.
17	N W		Cloudy and fair morn; hail showers, afternoon.
18	S to W N W	0·97	High wind, and showers.
19	S S W		A stormy day; heavy gale at night.
20	S S W	0·15	A gale, and showers.
21	W N W		Fair.
22	S to S E		Cloudy; high wind at night.
23	S to S S W	0·57	A gale of wind, and heavy showers.
24	S	0·25	High wind, and showers; a gale at night.
25	S S W to N W	0·07	Cloudy, and showers.
26	S S W to N		Ditto, ditto.
27	N W to N N E		High wind; cloudy and fair.
28	N E	0·12	Cloudy and fair morn; showers and high wind at night.
29	N E		High wind by day; a gale at night.
30	N E		A gale of wind; cloudy and fair.
31	N E		High wind; ditto, ditto.
		3·88	Inches rain.

			Wind.
Barometer:	Greatest height..................	30·20 inches	E
	Lowest	29·28	S S W
	Mean	29·763	
Thermometer:	Greatest height..................	67°	S
	Lowest	38	S
	Mean	52·661	

NOVEMBER.

Nov. 1	N E to N N W		Fair day; cloudy at night.
2	N W		Cloudy morn; fair day.
3	N W to N E		Fair day.
4	N E to S W		Hoar frost; fair day; cloudy at night.
5	N N W		Cloudy.
6	S S W	0·15	High wind; cloudy; showers at night.
7	W N W to N N W		Fog, morn; cloudy day.

Date.	Wind.	Rain.	Observations.
1815. Nov. 8	W N W	0·02	Cloudy; showers.
9	W N W		Ditto.
10	W N W		Ditto.
11	S S W	0·05	Misty.
12	W to S	0·77	Showers; a gale at night.
13	S to W		A storm; heavy showers of hail and rain.
14	W N W to N W	0·07	Hail showers.
15	N W		Sleet.
16	N W	0·30	Showers; snow ditto at night.
17	W N W to N W		Cloudy and fair.
18	N W to N E		Hoar frost; fog, morn; fair afternoon.
19	N E	0·10	Thick weather.
20	N E		Cloudy; high wind.
21	N E		Ditto, ditto.
22	N E		Cloudy and fair morn; fair eve.
23	N E to N W		Fair.
24	W N W to N E		Ditto.
25	N E		Cloudy and fair.
26	N E		Ditto.
27	N E		Ditto.
28	N E		Snow showers.
29	N E to S E		Cloudy and fair.
30	S S E	0·28	High wind, thick weather, and showers.
		1·74	Inch rain.

			Wind.
Barometer:	Greatest height	30·50 inches	N E
	Lowest	29·05	N W
	Mean	29·932	
Thermometer:	Greatest height	58°	W N W
	Lowest	26	N E
	Mean	41·30	

DECEMBER.

Date.	Wind.	Rain.	Observations.
Dec. 1	S S W	0·22	High wind, and showers, early; cloudy and fair day.
2	S W		Cloudy.
3	W		Fog, morn; cloudy and fair day.
4	W N W		Cloudy.
5	N W	0·48	Thick weather; a gale at night.
6	N W		A gale, and hail showers, early; showers during the day.
7	N W		High wind; cloudy and fair.
8	N E		Fair.
9	N E		Ditto.
10	N E to N W		Ditto.
11	N N W		Misty.
12	N W		Cloudy and fair.
13	N W	0·05	Ditto, morn; showers, aftern.
14	N W to S W		Cloudy and fair; an halo at night.
15	W to W S W	0·18	Thick weather, high wind, and showers.

Date.	Wind.	Rain.	Observations.
1815.			
Dec. 16	W N W	0·25	A gale early, morn; high wind, and showers, day.
17	W N W		Hail and snow showers.
18	N W		Hail showers.
19	Var.	0·95	Cloudy day, high wind, and heavy rain at night.
20	W S W		A gale, and heavy rain early, morn; wet day.
21	S W to N E		Cloudy.
22	N W	0·22	Ditto, morn; showers, afternoon.
23	W to W N W		Cloudy and fair morn; showers, afternoon; high wind at night.
24	W N W		Ditto, ditto.
25	W N W to N E		Snow showers.
26	S W	0·15	High wind, and showers.
27	W N W		A gale of wind; fair over head.
28	Var.	0·07	Misty.
29	W		Ditto.
30	W N W to E		Ditto; fair afternoon.
31	S to E		Cloudy.
		2·57	Inches rain.

			Wind.
Barometer:	Greatest height	30·47 inches	E
	Lowest	28·96	W S W
	Mean	29·811	
Thermometer:	Greatest height	53°	S W
	Lowest	21	N E
	Mean	38·241	

Article V.

A Comparison of the Old and New Theories respecting the Nature of Oxymuriatic Acid, to enable us to judge which of the two deserves the Preference. By Jacob Berzelius, M.D. Professor of Medicine and Pharmacy, and Fellow of the Royal Academy of Sciences at Stockholm.*

It is universally known that Sir Humphry Davy has given a new theory of muriatic acid and its compounds, which seems already to be generally embraced, though it has not remained without opposition. Hitherto I have not been able to see its superiority over the old theory; and on that account I consider myself as bound to state the reasons which induce me to adhere to the old opinion. This is the more necessary, as the reasons which could induce such distin-

* Translated from Gilbert's Annalen der Physik for Sept. 1815, Vol. L. p. 356.

guished men as Davy, Gay-Lussac, Vauquelin, &c. to adopt such opinions, must appear very strong; while their example must produce a great effect upon others. At the same time, I am aware that the obstinacy with which many philosophers adhere to old opinions is owing to their incapacity of perceiving the force of the arguments which are urged against them. The danger, however, of having this reproach thrown against myself, will not deter me from waging a war, from which, whatever should be the result, science must be a considerable gainer.

Sir H. Davy found that a piece of charcoal, brought by means of the galvanic battery to the most violent heat, was unable to decompose or alter dry oxymuriatic gas. Hitherto oxymuriatic acid had been considered as a loose compound of muriatic acid and oxygen. From the preceding experiment, it follows that this opinion is inaccurate. Davy now supposed, in consequence of this, that oxymuriatic acid is a simple substance, to which he gave the name of *chlorine*; and in order to demonstrate the truth of this opinion, he made oxymuriatic gas to act upon hot salifiable bases. The gas was absorbed, and a quantity of oxygen gas disengaged exactly equal to that which the salifiable basis contained. Hence he concluded that the oxygen did not proceed, as had been hitherto supposed, from the oxymuriatic acid gas, but from the salifiable basis itself. The impossibility of separating oxygen from oxymuriatic acid, by any method hitherto tried, he considered as a proof that the old opinion is wrong, and that, according to his own words, "chlorine must be regarded, according to a just logic of chemistry, as an elementary substance." (Elem. of Chem. Philos. i. 241.) Davy has since that time endeavoured to establish his opinion still more completely, and to confirm it by new proofs: and he now considers it as demonstrated that the old opinion is an improbable hypothesis, because it cannot be confirmed by experiment. It has not indeed escaped his observation that chlorine possesses properties which it would probably not have unless it were an oxydized body; but at the same time he lays it down (p. 485) that we are not entitled to infer from this that chlorine contains muriatic acid.

As the new opinion, and its apparent superiority over the old, depends chiefly upon these facts, I shall examine the degree of force which they possess.

It was expected, when charcoal was heated to redness in oxymuriatic gas, that carbonic oxide and common muriatic acid would have been formed. But the muriatic acid gas which we obtain is a compound of pure acid and water, just as concentrated sulphuric acid is a compound of real acid and water. Hence the reason why this expectation cannot be fulfilled; for the charcoal must either convert the oxymuriatic acid to muriatic acid destitute of water, or to muriatic radical. But if muriatic acid can exist only in compo-

sition, as may be inferred from analogy with several other acids; and if the radicle of muriatic acid has a greater affinity for oxygen than charcoal has, which is neither improbable, nor without example, then the preceding expectation could not be fulfilled, and yet it would not be necessary to infer that oxymuriatic acid is a simple body. This fact, therefore, though on the one side it seems favourable to the new opinion, yet on the other side it does not militate against the accuracy of the old opinion.

The impossibility of altering oxymuriatic acid by means of charcoal gave occasion to Davy's idea that this substance is simple. The fact, that when it unites with bases it disengages a quantity of oxygen exactly equal to what was contained in the base, he considered as a clear proof of the accuracy of that idea. We shall now, therefore, examine this proof.

Scientific propositions, which require to be proved, must be examined on all sides; because that which, seen only on one side, appears to be true, shows itself, when examined on another side, either quite inaccurate, or at least very doubtful. When Davy made these probable discoveries, his writings show that he was entirely ignorant of the experiments showing the constant proportions in which bodies unite. Since that time this branch of science has been much cultivated, and is, I conceive, fully established. It is necessary, therefore, to examine these opinions on the side of chemical proportions.

According to the old opinion, oxymuriatic acid is a super-oxide, and the separation of oxygen from it in the preceding experiments is the consequence of the greater affinity which the acid has to the base; just as by the action of sulphuric acid on the super-oxide of manganese, the excess of oxygen is separated, and sulphate of manganese formed. But, that the base of a super-oxide, capable of becoming an acid, should by the action of a base be reduced to an acid with the disengagement of oxygen, is in fact no more improbable than that the super-oxide of a substance capable of becoming a salifiable basis, should by the action of an acid be reduced to this basis with the same disengagement of oxygen. The quantity of oxygen disengaged in such cases must bear a determinate proportion as well to that in the acid as to that in the base; and this proportion may be easily determined by the analysis of some compounds of the acid and the base. Now when we determine the compounds of muriatic acid, and the proportions of their constituents, we obtain as a result that if oxymuriatic acid, according to the old opinion, be a compound of muriatic acid with an excess of oxygen, which it gives out when it combines with salifiable bases, this oxygen must be just the same which euchlorine gives out when it is converted into oxymuriatic acid; that is to say, $\frac{1}{6}$th of the quantity which hyper-oxymuriate of potash gives out when heated to redness, $\frac{1}{2}$ as much as must be contained in pure muriatic acid, and just as much as each of those bases contains with which the muriatic acid in the oxymuriatic acid is saturated. Consequently when oxymuriatic acid gas is absorbed by a hot salifiable basis, it must give out

just as much oxygen as the basis uniting with the muriatic acid contains. Thus it appears that the circumstance which has been advanced as a clear proof of the inaccuracy of the old theory is a necessary consequence of the precision of that theory, and consequently affords no sufficient reason for rejecting that theory.

Even if we allow that the non-reduction of oxymuriatic acid by means of charcoal gives in the present state of our knowledge probability to the new opinion, still it cannot be considered as established upon evidence which the science will admit as decisive. Though Davy considers it as evident that the oxygen comes from the base because it is equal to what the base contains in quantity, the advocate for the old opinion is entitled to assert that it comes from the acid, and that, according to the laws of definite proportions, it must be equal to the quantity contained in the base. It is the duty of the partisans of each opinion, therefore, to refute that of their antagonists by unequivocal experiments; and as long as neither party can do this, neither the one nor the other can be considered as established.

I conceive that by these preliminary observations I have shown that the facts established by the new opinion are not inconsistent with the accuracy of the old one. It is, therefore, clear that, in order to account for appearances, it is not necessary to have recourse to any other explanation than what is furnished by the old opinion. I have thought it necessary to state these things, in the first place, that the reader may not forget that he is not obliged by the phenomena to embrace one opinion more than the other, but is at liberty, after the comparison between them which I shall draw, to embrace at pleasure either the one or the other.

I shall now review the most remarkable compounds of *muriatic acid, fluoric acid,* and *iodic acid*; but I shall dwell principally on the *muriatic acid*; and as Davy's opinion appears to be almost universally adopted, I shall follow the order which it points out.

I. Muriatic Acid.

1. *Chlorine is a Simple Body.*

It did not escape the sagacity of Davy that chlorine possesses many of the properties of oxidized bodies; particularly the property of forming a combination with water capable of crystallizing, which is not the case with any other simple body. He allows in consequence, as we have seen, the probability that chlorine contains oxygen; but will not admit that muriatic acid is one of its constituent parts. It cannot be denied that this property of chlorine is not a little unfavourable to the new opinion, while it is perfectly consistent with the old one.

2. *Chlorine is a Combustible, or a Body capable of uniting to Oxygen.*

Chlorine is capable of combining with oxygen in two proportions,

namely, one volume of chlorine with one or with five volumes of oxygen. The oxide is called *euchlorine*, and the highest degree of oxidizement is called *chloric acid.* In these compounds there are two very remarkable circumstances: 1. It is very striking that chlorine, an elementary body, should be so very similar in its properties, as colour, smell, solubility in water, &c. to its first oxide, that for many years they were not distinguished. This fact is unfavourble to the new opinion; for it is reasonable to suppose that two so similar bodies are contiguous oxides of the same base. 2. The leap from one volume to five, which chlorine makes at once, when we compare it with other combustible bodies, is without example. We are acquainted with no oxides in which one volume of the base is combined with five volumes of oxygen; and from what we know of the corpuscular theory, we may conjecture that such a compound does not exist.*

The degrees of oxidation of chlorine, according to the old opinion, corrected by means of the doctrine of definite proportions, are as follows: 1. *Muriatic acid*, composed of 1 volume base + 2 volumes oxygen. 2. *Oxymuriatic acid* (*super-oxidum muriatosum*), composed of one volume base with three volumes oxygen. 3. *Euchlorine* (*super-oxidum muriaticum*), composed of 1 volume base + 4 volumes oxygen. 4. *Hyper-oxymuriatic acid* (*acidum oxymuriaticum*), composed of 1 volume base + 8 volumes oxygen. These not only agree perfectly with each other, but are likewise in the greatest harmony with the definite proportions of muriatic acid in the simple, double, neutral, and sub-salts. From the old opinion, likewise, it may be deduced that the *bleaching liquor* obtained by passing oxymuriatic acid gas through a diluted solution of caustic potash is a compound of the base of muriatic acid with six volumes of oxygen, or an *acidum oxymuriatosum*; for that it is not a compound of oxymuriatic acid and potash, is evident from the quantity of common muriate of potash which is formed. From the above observations, it is obvious that the new opinion does not accord so well with the doctrine of definite proportions as the old one.

3. *Chlorine has a stronger Affinity for Combustible Bodies than Oxygen, and therefore separates them from that Substance.*

The electro-chemical discoveries of late years have rendered it highly probable that chemical affinity depends upon the electro-chemical properties of bodies, and is the greater the greater the electro-chemical opposition of the bodies united together. When a body separates from another by simple elective affinity, and at the same time occasions an increase of temperature, it is a proof of a greater affinity, which is always proportional to the increase of temperature; and the increase of temperature itself appears to proceed from a

* That nitric acid, which, according to some chemists, contains only five volumes of oxygen, in reality contains six volumes, I have shown by experiments, which cannot be inaccurate, unless a number of other experiments, very easily performed, be likewise erroneous.

more complete destruction of the electro-chemical opposition in the bodies united together. Thus, for example, potassium separates the metal from oxide of copper with the appearance of fire, but from the oxide of iron only with an increase of temperature; because iron has a stronger affinity for oxygen than copper has, and therefore destroys the electro-chemical properties of oxygen more completely than copper (though potassium destroys them still more completely).

Potash free from water (obtained by the action of potassium on the peroxide of potassium) exposed to the action of oxymuriatic gas, absorbs it with an increase of temperature, which when the potash has been previously heated amounts to the appearance of fire. The same thing takes place with the hydrate of potash, though in a less degree. By this absorption oxygen gas is set at liberty. If this oxygen proceeds from the potash, the experiment would show that chlorine has a stronger affinity for potassium than oxygen has, and therefore that it destroys the electro-chemical properties of potassium more completely than oxygen does; and consequently that chlorine is a more electro-negative body than oxygen. But chlorine constitutes the basis of euchlorine and chloric acid, that is, the electro-positive ingredient; therefore chlorine is less electro-negative than oxygen; but it is self-evident that it cannot be at the same time both more and less negative than oxygen. Therefore the separation of the oxygen from the potash in these experiments is inconsistent with the electro-chemical doctrine. Hence it clearly follows that either the electro-chemical doctrine, or the new opinion respecting the nature of chlorine, must be false. On the other hand, according to the old doctrine, it is very conceivable that in the super-oxide of muriatic acid the acid allows the excess of its oxygen to escape, in order to combine with the salifiable basis, for which it has a greater affinity. As the quantity of oxygen in the new compound remains the same, the separation proceeds entirely from the affinity of the radical of the basis of the salt. Hence it follows that the explanation afforded by the old theory is perfectly satisfactory, and agrees with the other parts of the theory of chemistry.

4. *Chlorine combines with Sulphur, and forms a Chloride of Sulphur.*

The *chloride of sulphur* is the *muriate of sulphur* discovered by Thomson; and, according to the old doctrine, it must be a compound of muriatic acid and the oxide of sulphur. With respect to this compound, the new doctrine appears at first sight to have a great superiority over the old one, because it is not under the necessity of admitting the existence of one oxide of sulphur no where else to be found. Yet such a supposition is by no means improbable. We are acquainted with several bodies not capable of existing insulated in a particular degree of oxidation, and which are decomposed whenever we attempt to procure them in a separate state.

If this be the case with the oxide of sulphur, we need not be surprised that it can only be exhibited in a state of combination. When the muriate of sulphur is decomposed by water, all the oxygen of the sulphur combines with one-half of the sulphur, and forms sulphurous acid; while the other half of the sulphur appears to be reduced. According to the new doctrine, the water is decomposed, the hydrogen forming with chlorine muriatic acid, and the oxygen with a portion of the sulphur, sulphurous acid. The superiority of the new doctrine, therefore, consists in this, that it has no occasion to suppose the existence of a lower degree of oxidizement of sulphur (an opinion in other respects not improbable). But we shall soon destroy this apparent superiority, and turn the weapons of the new opinion against itself.

The substance discovered by Dr. Marcet and myself by the action of aqua regia on sulphuret of carbon is sufficiently known. According to the old doctrine, it must be considered as a combination of three acids free from water, namely, muriatic acid, sulphurous acid, and carbonic acid. The quantity of oxygen in the last two is equal to each other; and that in the muriatic acid as great as in both the others. According to the new doctrine, this body must consist of one proportion of *phosgene* (the name given to the body formed by the combination of carbonic oxide and chlorine) and one proportion of a compound of chlorine, sulphur, and oxygen. But the proportion of sulphur and oxygen in this compound is quite the same as in the oxide of sulphur admitted by the old doctrine in the muriate of sulphur (the sulphur is combined with half as much oxygen as in sulphurous acid). Hence it is obvious that the new doctrine must admit the existence of the same oxide in order to explain the nature of the compound in question. The new doctrine, therefore, in this point of view, has no superiority whatever over the old doctrine.

5. *Chlorine combines with Phosphorus in two Proportions.*

The compounds formed by the action of oxymuriatic acid on phosphorus are, according to the old doctrine, compounds of muriatic acid and phosphorous or phosphoric acids free from water. When water comes in contact with them, it decomposes them, and reduces them to the state in which they exist when united with water. But according to the new doctrine these compounds are peculiar acids free from water, in which the phosphorus constitutes the base, or the electro-positive; and chlorine, the electro-negative ingredient. These acids are capable of uniting only with one base, namely, ammoniacal gas. By all other bases they are decomposed, a phosphate and a chloride being formed. The old doctrine appears to me in this case to be both simpler and more correct; as it considers the ammoniacal salt as a double salt without water, and composed of two acids united to one base; and the compounds of these two acids with other bases, either as equally double salts, or as mix-

tures of phosphates and muriates, formed according to circumstances, either with or without chemically combined water.

6. *Chlorine does not combine with Charcoal, but it unites with its own Bulk of Carbonic Oxide Gas.*

From the phenomena peculiar to muriatic acid flows the old doctrine that this acid cannot exist in a separate state, as is the case also with nitric acid, oxalic acid, tartaric acid, and several others. If we suppose that charcoal has a weaker affinity for oxygen than the basis of muriatic acid has, oxymuriatic acid gas may only be reduced to muriatic acid when an oxide is present with which the acid may combine. If carbonic oxide were not unlike other oxides (as is in general the case with suboxides), it would combine with muriatic acid, and form a muriate of carbonic oxide quite similar in appearance to the combination of one volume of chlorine with one volume of charcoal. According to the old doctrine, we can in some measure see why charcoal does not act upon chlorine, while the new doctrine affords no explanation of the reason why charcoal is the only element incapable of uniting with chlorine without the intervention of oxygen.

When chlorine combines with its own volume of carbonic oxide, it forms a strong gaseous acid, which has obtained the very improper and intolerable name of *phosgene* gas. This acid, according to the new doctrine, is analogous to chloro-phosphoric acid, but distinguished from it by containing oxygen gas. It is the only example of an acid composed of one electro-positive body, charcoal, and two electro-negative bodies, chlorine and oxygen. This acid is capable of uniting only with a single basis, ammoniacal gas, and forming with it a salt. By all other salifiable bases it is decomposed, and there is formed a carbonate and a chloride. Phosgene, then, is a pretty strong acid, composed of one basis and two *oxygens* (*sit venia verbis*), which is capable of forming a salt only with one base; since with all other soluble bases it forms a very different compound of a carbonate and a chloride.

According to the old doctrine, oxymuriatic acid gas contains half its bulk of oxygen in excess. Therefore carbonic oxide gas finds in its own bulk of oxymuriatic acid gas a sufficient quantity of oxygen to convert it into carbonic acid. By the mutual action of the two gases, carbonic acid and muriatic acid gases are formed, which unite with each other, and form a double acid, in which both acids contain an equal proportion of oxygen. This acid unites both with bases free from and containing water. Some of these compounds are true triple salts, composed of one basis and two acids; others only a mixture of a muriate and carbonate. To the triple salts belong the ammoniacal salt, the lead salt, and perhaps many others, which may also be formed by a mixture of muriates and carbonates. For example, if moist carbonate of lead be introduced into a boiling hot and saturated solution of muriate of lead, the

two salts combine, and form an insoluble triple salt, composed of one basis and two acids. It appears to me that in this case also the old doctrine accords with our other chemical opinions.

(To be continued.)

Article VI.

On a New Compound of Phosphorus and Potash. By the Chevalier Sementini, Professor of Chemistry at Rome.*

The compound of potash and phosphorus remains still unknown. No mention is made of it in the modern books of chemistry; and Klaproth, in his Dictionary of Chemistry, says frankly that "the science has not yet pointed out a mode of combining that substance with phosphorus."

I have obtained, for the first time, the phosphuret of potash by the following process :—

1. I saturated very strong alcohol with potash. This solution had a deep amber colour; its consistence was, as it were, oily, and its taste very caustic.

2. I introduced into this liquid pieces of phosphorus, which appeared instantly covered over with bubbles of gas. The phosphorus diminished in quantity, dissolving as this gas, which was a protophosphuret of hydrogen, was disengaged. When the first pieces were entirely dissolved, I introduced new ones; and I proceeded in this manner till all disengagement of gas was at an end, and till the phosphorus ceased to dissolve in the liquid. This saturation required at least 15 days.

3. At the end of this first operation I found at the bottom of the vessel a powder of a dark red colour, and scales of a certain brilliancy, but covered with that powder.

4. I separated the sediment of the liquid by the usual method of filtration; and the liquid appeared to differ from the alcoholic solution of potash by its straw colour; by its consistence, which was no longer oily, but fluid, like water; and by its taste, which had become sweetish and sharp.

5. I dissolved the scales that remained on the filter in pure water. The solution was muddy; but, on being filtered, it became limpid, like water.

6. There remained upon the filter a red powder, similar in appearance to kermes mineral.

7. The sides of the vessel in which the solution of the phosphuret had been made remained stained with black.

* Translated from the Bibliotheque Britannique, Sept. 1815, vol. lx. p. 24.

8. The liquid of paragraph 4, which was exposed to the air, became, after some days, covered with a yellowish pellicle, of an oily appearance, which disappeared after some weeks. When this substance was separated from the liquid, it assumed the appearance of No. 5.

9. The two liquids (Nos. 4 and 5), being evaporated to the consistence of a syrup, yielded confused, and not permanent, crystals. Evaporated to dryness, they gave the phosphuret of potash under the appearance of a white opake mass, which, when strongly heated, burnt with a yellow flame. The residue of this combustion was a grey mass, semi-liquid, and deliquescent, and in its inside partly yellowish, partly blackish.

10. The scales remaining on the filter are a true phosphuret of potash, and do not differ from the liquids 4 and 5, but by the difference of the solid and liquid state. They deliquesce when left exposed to the air; and, when heated till they become dry, they take fire, and burn with a white flame.

11. The red matter of No. 6 becomes darker coloured when left exposed to the air, but it remains humid and coherent. When treated with perchlorate (*hyper-oxymuriate*) of potash, it gives phosphorous acid gas, carbonic acid gas, and carbonate of potash.

From an attentive examination of all these facts, it appears that they are the result of a play of affinities, in consequence of which the greater part of the phosphorus combines with the potash. The compound divides itself into two portions, one of which precipitates under the form of scales, and the other remains in solution. The solution of the phosphuret of potash (No. 4) contains, likewise, a certain quantity of alcohol of phosphorus; and the yellowish pellicle with which the liquid becomes covered, when left exposed to the air, is owing to this, that the alcohol, on evaporating, leaves the phosphorus in a state of extreme division, which enables it gradually to disappear when left in contact of air.

Besides the principal compound, the phosphuret of potash formed in this process, others are likewise produced. The alcohol is decomposed, its hydrogen is developed in the state of gas, and dissolves at the same time a portion of the phosphorus. The carbon of the alcohol combines with the phosphorus and potash, which produces the reddish-brown matter of No. 6, which is a triple compound of phosphorus, potash, and charcoal, as appears from the products when it is treated with perchlorate of potash, No. 11.

The two solutions, Nos. 4 and 5, when burnt as described in No. 9, contain likewise a portion of oxide of phosphorus, from which proceeds the yellowish and blackish colours which alternate in the residue of this combustion.

I proceed to the action of the acids on the solutions of the phosphuret of potash of Nos. 4 and 5.

Hitherto the action of three acids only, the sulphuric, muriatic, and nitric, has been tried. They only produce an imperfect decomposition of the phosphuret of potash. Sulphuric acid deprives the

phosphuret of potash of the greatest part of its alkali; and the two liquids are converted into a gelatinous mass, which is a mixture of sulphate of potash and super-phosphuret of potash. This mixture, when heated till it becomes dry, exhibits the important phenomenon of the complete decomposition of sulphuric acid; a result which cannot be obtained completely by any other agent than phosphorus. From this decomposition it comes to pass that the heated mass, before it be quite dry, acquires a yellow colour from the sulphur disengaged. This sulphur is gradually dissipated in the state of vapour. The saline matter resulting from this decomposition is phosphate of potash.

Muriatic acid poured into the liquid occasions the precipitation of muriate of potash, which cannot be separated from a certain quantity of phosphuret of potash that remains combined. Hence when the mixture is heated it takes fire, in consequence of the portion of phosphuret of potash which it contains.

Nitric acid produces nearly the same effect on the liquids of Nos. 4 and 5. The nitrate of potash which is formed crystallizes pretty rapidly, and is deposited at the bottom of the liquid. When entirely separated from the supernatant liquid, and strongly heated, it produces the same effects as common nitre. But if the mixture of liquid and salt be heated till it becomes dry, one of the most violent and dangerous detonations takes place to which we are exposed in chemistry. The experiment must be made upon a very small quantity of matter, otherwise the danger will be serious. Not being aware of the effect, I evaporated to dryness about 36 grains of the salt mixed with the liquid. The detonation was so violent as to drive me to the ground, quite confused; and I remained deaf during a whole day. Every thing brittle on the table of the laboratory was broken to shivers. Considering the small quantity of matter capable of producing such an effect, I conceive that this substance, next to detonating silver, is the most violent with which we are acquainted.

The phenomena which I have described are doubtless entitled to a more accurate examination, and I propose to study them more in a set of experiments which I intend to undertake; hoping that in the mean time I shall receive such information from those chemists who are interested in the subject as will enable me to advance further in that inquiry.

P. S. I afterwards found a more convenient mode of obtaining phosphuret of potash. It is as follows:—Form a saturated solution of potash in water. Add pieces of phosphorus. No reciprocal action takes place; but when highly rectified alcohol is added, a lively effervescence immediately takes place, and proto-phosphureted hydrogen gas is evolved in abundance. The phosphorus dissolves with the same rapidity at the commencement of the operation, but more slowly afterwards. The phenomenon does not proceed from the small elevation of temperature produced by the mixture of the alcohol with the water; for when potash and phosphorus are heated

together, we obtain no direct combination between them; but phosphureted hydrogen gas is disengaged, and phosphate of potash remains behind in the liquid. Hence the affinity of the alcohol is indispensable to produce phosphuret of potash.

The aphrodisiac property well known to belong to phosphorus induced me to try if the phosphuret of potash possessed the same effects on animals; and my first attempts promised complete success. If I succeed in establishing this effect, this property of phosphorus may be taken advantage of, without being exposed to the inconveniences so frequently resulting from the direct use of this substance. I propose to publish in a particular memoir the manner of administering this remedy; the precautions requisite to prevent the decomposition of the phosphuret of potash in the stomach, and to add the cases in which it has been employed with more or less success.

Article VII.

On the Ventilation of Coal Mines. By Lieut. James Menzies, of the 68th (or Durham) Regiment of Foot. With a Plate.

(To Dr. Thomson.)

DEAR SIR, *Newcastle-on-Tyne, Nov.* 13, 1815.

In my last letter I hinted to you that I had for some time back been endeavouring to arrange what I conceived might be an improvement in the ventilation of coal-mines; and that I had been interrupted by the arrival of a Gentleman, who, it was said, had a new system of ventilation to offer, which possessed the decided advantage of having been already tried in the Staffordshire mines with good effect.

I have now learned that his plans of ventilation have been rejected; that *two other* plans were deliberated upon at the same time, one of which had the preference; but whether with the intention of adopting it, I have not ascertained. I may venture, however, to predict that no *new* plan of ventilation will take place in this quarter for some time to come.

It too frequently happens, when a person has made, or fancies he has made, a discovery of so much importance as a perfect system of ventilation must unquestionably be, that he also fancies it to be so *extremely simple* as to require some degree of secrecy, lest others should take advantage of it to his detriment; and at the same time so *extremely perfect* that little more is requisite than to stipulate upon what terms it may be employed.

However well founded these ideas may be in particular cases, so far as relates to the ventilation of coal-mines in the north of England they are completely erroneous. There are strong deeply

rooted prejudices here, of more than 50 years undisturbed growth, in favour of the existing system of ventilation, which the influence of no individual, be his genius what it may, can ever remove. These prejudices can only be obviated by the collected force of public opinion. The public has at last been clearly convinced that the present system of ventilation is miserably defective. Whenever it shall be as fully satisfied respecting the efficacy of any particular remedy, that remedy must, and will, be applied. There can be no question, then, that those persons who think proper to conceal their discoveries on this important subject, expose themselves to the most mortifying disappointments, by rejecting, or by neglecting to secure, assistance, which, when given, never fails to effect its purpose.

These observations may be illustrated by the circumstances already alluded to. We find that two or three plans of ventilation have been formed, and kept secret; that *two* have been rejected; and that *one* has received that cold, doubtful sort of approbation, which is equal to, if not worse than, contempt. Let us hope that this example will induce every person who has turned his thoughts to the subject of ventilation to come forward, and lay before the public, without hesitation or reserve, every thing which he may conceive likely to prevent the recurrence of those tremendous accidents, the frequency of which more than sufficiently proves that scarcely any *new* system of ventilation can be more destructive of human life than the old.

Under these impressions, I propose, if you can spare so much room in your Journal, to lay before your readers the outlines of a plan of ventilation, the arrangement of which has for a considerable time back occupied my leisure time, until it was interrupted, as I have already stated, by the hope of seeing a remedy immediately applied: I say *arrangement*, for I am not sure that I shall have much claim to invention,—certainly none at all to discovery.

My plan is constructed upon two fundamental principles, or well-ascertained facts. The first of these, viz. *the dip or declination from the horizontal line* of all the strata of which the crust of the globe is composed, is mentioned by every author who has written on mineralogy. It is, indeed, the leading fact of the science, upon which numerous practical operations depend; though there are none of more importance than the additional one, which it is hoped may be founded upon its application, as a principle, to ventilation.

In a small work, published here in 1809, by Mr. Westgarth Forster, entitled, A Treatise on a Section of the Strata commencing near Newcastle-upon-Tyne, &c. the author begins by describing the stratification of coal: and as this description contains almost all that is necessary to be known, so far as relates to the present purpose, it will be proper to extract and connect such parts of it as may assist in giving your readers a clear idea of the facts.

At p. 9, he begins by explaining "the term *strata* in natural history" to signify "the several beds or layers of stone, or various substances, whereof the solid parts of the earth are composed.

These strata consist of various kinds of matter, as free-stone, lime-stone, indurated clay, coal, &c. and are disposed in beds or layers, the under surface of one bearing against, or lying upon, the upper surface of the inferior stratum, which last lies or bears against the next below in a similar manner. Some of these strata are of considerable thickness, being often found from 60 to 100 feet, or upwards (nearly uniform) from the upper to the under surface."

At p. 11, he goes on to say that "a seam or bed of coal is a real stratum, which is found to be fully as regular as any of those other concomitant strata found in the coal-field, lying above and below the coal; or, indeed, of any other of the various strata which compose the superficies of our globe.

"There are in many coal countries, and in many coal-fields, a considerable number of strata or beds of coal, of various qualities and thickness, placed *stratum super stratum*, with a great variety of other strata interposed between them; and sometimes different strata, or seams of coal, are so near to one another, that two, three, or more of them, are cut through, and worked in one pit.

"Every stratum of coal has some degree of declivity or slope, together with a longitudinal bearing; and it stretches as far every way as other strata which accompany it.

"The strata are seldom or never found to lie in a true horizontal situation, but generally have an inclination or descent, called the *dip*, to some particular part of the horizon. If this inclination be to the eastward, it is called an *east dip*, and a *west rise*; and, according to the point of the compass to which the dip inclines, is the denomination. The ascent, or rise, is to the contrary point. This inclination, or dip, of the strata, is found to hold every where. In some places it varies very little from the level; in others, very considerably; and, in some, so much as to be nearly vertical.

"But whatever degree of inclination the strata have to the horizon, if not intercepted by *dykes*, *hitches*, or *troubles*, they are always found to lie in the regular manner first mentioned. They generally continue upon one uniform dip until they are broken or disordered by a dyke, a hitch, or a trouble. If the strata have an east dip, they may, by the intervention of a dyke, have on the other side an east rise, which is a west dip; and in general any considerable alteration in the dip is never met with unless occasioned by the circumstances last mentioned.

"Every stratum in a whole range, or class, or coal-field, is spread out to a vast extent in an inclining plane, suppose of a mile, or of several miles square, like an inclining field, or face of a country. A dead level line drawn through that inclining plane is called the *bearing of the strata*; and another line, drawn right across the dead level line, or bearing, is called the *declivity of the strata*, or the dip and rise of the strata. In general we can see but a very little way from the rise to the dip, or along the line of declivity of the strata, because the strata soon dip down out of our sight, and generally a great number of other strata come on above them. But,

on the contrary, we can sometimes trace the same individual stratum, or number of strata, along the surface, or dead level line, for several miles; and, therefore, we may properly call this the longitudinal line of bearing.

"From these observations it appears that every individual stratum in the whole section keeps its station where you see it placed; and that it spreads as wide, and stretches as far, as any of those which are placed above and below it, which, perhaps, may be for several miles every way."

"The stratum which is placed immediately above the seam of coal is properly called the *roof* of the coal; and the stratum which is placed immediately below a seam of coal is with equal propriety called the *pavement* of the coal. Now these three, that is, the stratum of coal, its roof, and its pavement, with the other concomitant strata lying above and below them, always preserve their stations and parallelism; that is, they are all stretched out, and spread one above another, upon the same inclining plane; and they have the same lines of bearing and declivity."

To the foregoing may be added, as entering a little more into detail, the following passage from the Edinburgh Encyclopedia, vol. vi. part 2d, under the article *Coal*:—

"Coals, with their various accompanying and parallel strata, are found lying in every inclination to the horizon. Some of them are vertical, others nearly horizontal, but never absolutely so, to any considerable extent. The *most common dip or declination is from* 1 *in* 5 *to* 1 *in* 20."

From the description just quoted, your readers will probably be able to acquire a correct notion of the general position of the coal strata; and in order to form a tolerably precise idea of a coal-mine in *this* neighbourhood, before it has been opened, they have only to suppose a portion of this inclining stratum of coal to be "spread out like the face of a country," to the extent of 1200 yards square; that it is six feet thick from the "roof to the pavement;" that it dips to the south-east, consequently rises to the north-west; and, further, that the south-eastern side of this supposed square of 1200 yards is 90 fathoms, or 180 yards in perpendicular depth, while the north-western side is only 80 yards below the general surface of the country. This is taking the dip at the rate of 1 in 12, nearly the mean of the extremes quoted from the Encyclopedia, and which will be found to correspond with the dip of several collieries now working.

Having thus exhibited a field, stratum, or seam of coal, in its extent and position in the bowels of the earth, it is necessary to state, for the information of such as are not much conversant in the details of coal-mining, that there are many very formidable obstacles to be encountered and obviated, before it be brought to the surface from such a depth. The sinking a *shaft*, or pit, 180 yards in depth, chiefly through rocks, many of which are of the hardest description, might seem to an uninformed person to be the principal

difficulty. To the proprietors of the mine it is indeed a most serious one. The expense is enormous, and success, after all, uncertain. This difficulty, however, rests here: the coal can be got at with little, if any, danger to the lives of the persons employed in finding it.

Obstacles of a very different nature are met with when the coal is found; they are generally in attendance, and commonly present themselves along with it. These consist of *water*, which must be extracted as fast as it comes in: of two kinds of *air*, one of which, if inhaled, instantly suffocates, from which deadly property it has obtained from the miners the significant name of *choak damp*. By chemists it is known to be *carbonic acid gas*. The other kind of air possesses destructive powers far more extensive. This formidable compound in certain circumstances takes fire at the flame of the miner's candle, and explodes with such irresistible force as to dash in pieces men, horses, waggons, or whatever may happen to be within its range, which not unfrequently extends through all the workings of the mine.

This terrible enemy to miners is called by them *fire-damp*; above ground, it is commonly called *foul air*, which term, it may be remarked, is extended to all sorts of air which are known to be noxious or fatal. By chemists this air is termed *carbureted hydrogen gas*. How it is generated has but little reference to the object of this paper. It is sufficient to observe, that it is found in the *workings*, or cavities left by the workmen in bringing out the coal; that, if not removed, it gradually, and sometimes rapidly, accumulates; and that its firing has often produced most deplorable effects.

This fire-damp, foul air, or carbureted hydrogen gas, has been examined by philosophers; and its distinguishing properties have been ascertained by experiment. The most striking of these, viz. *inflammability*, is found to be restricted to certain conditions. One is, that atmospheric, or common air, must be present; otherwise it will not take fire at all. Another, that, though it will *burn* in open air when lighted, as we see in gas lights, yet it will not *explode* unless it amounts to $\frac{1}{12}$th part of the bulk of common air, with which it must also be *confined* in some way, so as to be under compression.

Another property of this air is no less remarkable; and that is its *levity*, or comparative *lightness*. It is among the lightest of all known substances. Any bulk of it has only about half the weight of an equal bulk of common air. Hence its tendency to ascend, or rather to be forced upwards, by the pressure of the atmosphere; as a cork, when placed at the bottom of a vessel of water, is forced upwards by the superior weight and pressure of the surrounding fluid.

This property was lately exemplified in the ascent of Mr. Sadler's balloon from Newcastle, which was filled with a gas nearly of the same qualities as that which infests more or less the collieries in the neighbourhood.

But this peculiarity was doubtless impressed on this substance for other and more important purposes. It presents itself as the means of getting rid of the gas in situations where it is evolved, or accumulated in dangerous quantities; and with the intention of assuming this striking property of inflammable air as a fundamental principle in ventilation, it is necessary to adduce some evidence in support of what has already been said concerning it.

In the *Annals of Philosophy* for the month of June, 1814, at p. 435, the following passages occur:—

"I wish the proprietors of coal-mines would turn their attention to some circumstances which, if duly attended to, would enable them completely to put an end to the disastrous effects of explosions of fire-damp in their mines. These circumstances are the following: — 1. Explosions of fire-damp are confined to deep coal-mines, and never happen in those at no great distance from the surface of the ground. Thus nobody ever heard of such explosions in the neighbourhood of Edinburgh or Glasgow; but about Borrowstoness, where the mines are deep, they occur as well as in England. 2. The specific gravity of carbureted hydrogen gas is only 0·555, or a very little more than one-half of the specific gravity of common air. 3. If you let go ever so much carbureted hydrogen gas in a room with an aperture at the roof, and examine the air of the room half an hour after, no traces of the carbureted hydrogen will be detected. 4. Carbureted hydrogen will not explode unless it amounts to $\frac{1}{12}$th of the bulk of the common air with which it is mixed. The unavoidable conclusion from these facts is, that if the fire-damp accumulates in coal-mines so as to explode, it is only because circumstances prevent it from making its escape with sufficient rapidity. Hence it follows that the defect lies in the mode at present employed to ventilate coal-mines; and that if the mines were ventilated according to the well-known principles of hydraulics, no explosions would ever take place.

"To prevent the evolution of fire-damp I conceive to be impossible; to attempt to destroy it when formed, as has been sometimes proposed, is quite absurd; but allow it to make its escape from the mine without obstruction, and it will occasion no inconvenience whatever."

Having so far explained the principles to be employed, it now remains to show how they may be combined, so as to secure their constant operation in "letting" the fire-damp "escape with sufficient rapidity," and "without obstruction."

Referring, then, to the description of the strata already given, your readers will readily perceive that Fig. 1, Plate XLVII., is intended to represent a perpendicular section of a field of coal in the direction of the dip and rise, in which B, C, D, show pretty nearly the direction of the inclining stratum of coal, though the thickness is enlarged beyond its due proportion, on account of the smallness of the scale.

A represents a pit, or shaft, sunk to the coal on its south-eastern,

Fig. 1.

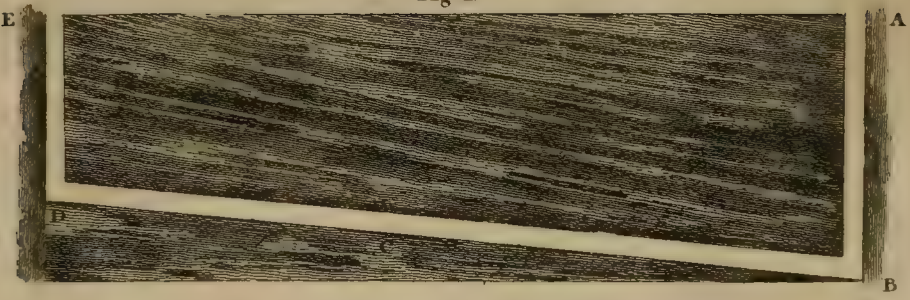


Fig. 2.

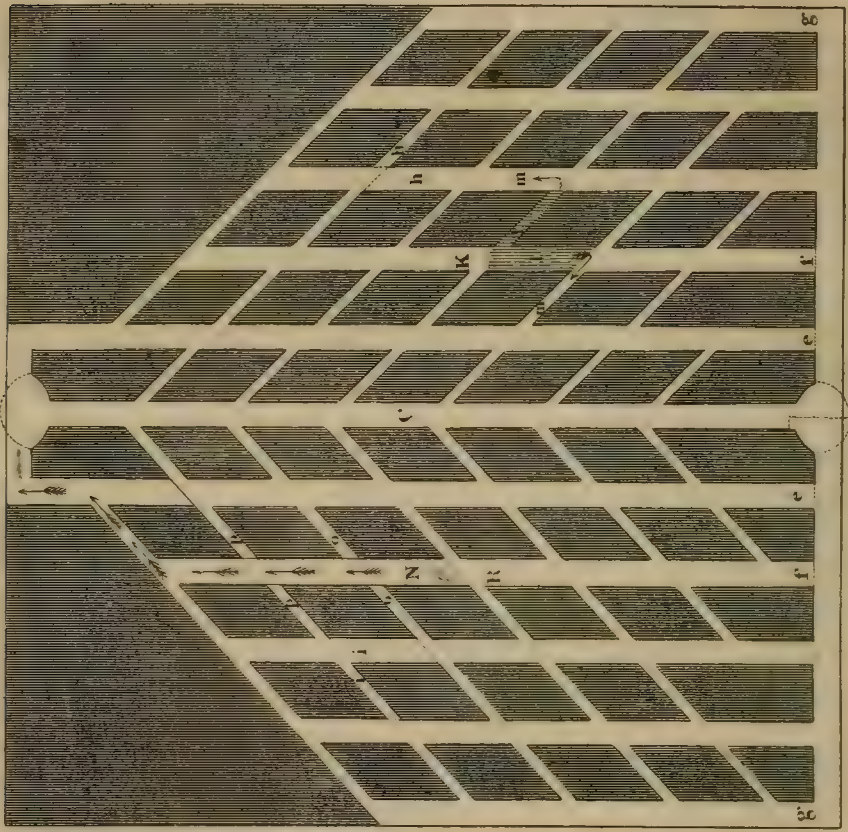
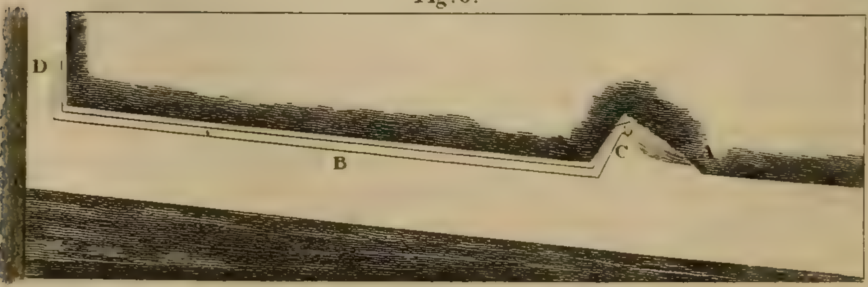


Fig. 3.



VENTILATION of COAL MINES.

Engraved for Dr. Thomson's Annals, for Baldwin Cradock & Joy Paternoster Row, April 1. 1816

or *lower* edge, which shaft is 180 yards in depth. E is another shaft, distant 1200 yards from A, which is also sunk to the coal on its *higher*, or north-western, side. If the coal be supposed to rise from B to D at the rate of one yard in twelve, then D must be 100 yards higher than B; consequently the depth of the shaft, E, from the surface will be 80 yards.

It has already been observed that it is possible to sink pits to these or to greater depths, without danger to the lives of the workmen. It is now to be added that it is also possible to open a communication below ground, between pits situated at considerable distances from each other; and that when two or more pits are sunk for one mine, the first operation is to open such a communication.

Let it be supposed, then, that a communication between the shafts A and E has been opened in the usual way, by digging a passage through the coal from B to D. The whole passage, A, B, C, D, E, will then resemble a tube, of which the two divisions, A, B, and E, D, are perpendicular to the horizon, and of which the middle part, B, C, D, has a considerable angle of declination from it.

It would seem to be evident, *à priori*, that a current of air, if it should move at all, without mechanical impulse, would proceed, in a tube of this form, and in this position, from A to E, and not from E to A; but this may easily be subjected to experiment.

1. Let a tube be constructed of any material which conducts heat slowly, as glass, for instance. Let it be made exactly of the form represented in Fig. 1, and placed precisely in the same position with respect to the horizon. Let heat be applied to any part of the middle division, B, C, D, by the flame of a lamp, or otherwise, and a circulation of air will commence, and proceed from A to E, which may be made evident by placing a little down of feathers over each opening of the tube: the ascending air will carry it upward from E, while the descending current will press it downward into A. 2. Let the tube be filled with carbureted hydrogen gas, or fire-damp. The same effects will follow, *without* the application of heat, or of any mechanical impulse whatever. Apply heat, as before, and it will increase the rapidity of the current. 3. Fill the tube with any proportions, equal or unequal, of fire-damp and common air: the current will still take the same direction, and will still be quickened by the application of heat.

These are all the cases that can possibly occur, involving the chances of danger from inflammation, or explosion, in the passage A, B, C, D, E, considered as the first openings of a coal-mine; for this passage must either be filled with common air, with fire-damp, or with certain proportions of each with the other. But with whichever of these kinds or mixtures of air it may be filled, it *must circulate*; because, first, the temperature is known to be in general higher below ground than at the surface, which would *begin* the circulation; and, secondly, because B, being 100 yards deeper than D, would *continue* it in the direction C, D, E.

So long, then, as there is only the passage B, C, D, between the two shafts, the ventilation of this passage is an operation of great simplicity; but this passage is only the *base line*, as it may be called, from which numerous excavations are to proceed in different directions, as far as the right of working extends, or as far as it may be possible to carry them with safety. Hence it is obvious that, as the space thus hollowed out below ground enlarges, the atmospheric current ought to flow through every part of the workings, to enable the workmen to breathe, to supply air for the necessary quantity of light, and to prevent the fire-damp from stagnating in dangerous quantities.

To give your readers an idea of the mode in which this may be effected, Fig. 2 represents a ground plan of the coal-field, of which Fig. 1 is a perpendicular section. In this figure, B is the *downcast*, or lower shaft; D, the *upcast*, or higher shaft; and C, the passage connecting them, as already shown in Fig. 1. The shaded rhomboidal figures are the *pillars* or masses of solid coal, left standing to support the roof; and the parallel and oblique openings between the pillars are the *workings*, or excavations, left empty by removing the coal; and through which an adequate supply of atmospheric air must be constantly directed, to ensure the safety of the mine.

It is a fact well known, and particularly by miners, that the current of atmospheric air in a mine will, when unobstructed, always take the most direct passages it can find, from its entrance to its outlet. Hence the direction of this current when introduced at B, the downcast shaft, would be along the passage C, to the upcast shaft at D. This, however, would not answer the purpose of ventilating the other parts of the mine; but if we stop the passage C at or near to the shaft B, we shall turn the current into the two next passages, *e*, *e*, on each side of B. If we want to carry it further, it is only necessary to stop the passages, *e*, *e*, and then will it proceed to the next pessages, *f*, *f*, and so on, by similar means, to the extremities of the workings, *g*, *g*. To distribute this current equally the shaft B may be divided by a partition across it; but in practice it will be found to divide itself accurately, in proportion to the demand opened for it.

It is not, however, to be understood, that the *whole* of the circulating current is to be introduced into any one passage, unless on some very particular occasions. In the ordinary circumstances of the mine, it must be carefully distributed through all the passages by a judicious application of *stoppings*; and these must be so constructed as to admit the necessary supply of air to the passages in which they may be placed.

Nothing can answer so well for a stopping as a door, hung on hinges from the roof, so as to swing freely in either direction, and so placed, that, when at rest in its perpendicular position, it may accurately shut up the passage. It is obvious, that by opening such a door more or less, the current of air through the passage in which it stands may be regulated at pleasure; and that it may be fixed at

the point which has been found to admit the requisite supply.* These doors, or stoppings, may be made of light materials, as they are not intended to withstand the application of any considerable force. In placing them there is only one rule to be observed, which is, that they must always be fixed in the *lower* opening of the passage, with regard to the general dip of the mine, and not in any higher part of it. Stoppings for instance, must not be put into the upper ends of the passages *h*, *h*, but into the lower extremities, as in the similar passages, *i*, *i*. While these precautions are attended to, the stoppings can never, by any accident, become the means of accumulating the fire-damp.

Recollecting then the rise of the mine, and the levity of the fire-damp, an attentive examination of the figure will show, that if the stoppings be judiciously placed, the passages clear, and the shafts open, no atmospheric air, much less inflammable air, can possibly stagnate, or accumulate, in any part of the workings. It will be observed, that the air, in this mode of ventilation is *never turned down the slope*, after having once ascended it; but that it invariably proceeds *upward* from the moment it begins to ascend, until it is discharged by the upcast shaft at D. It will also be evident, that the circulating current is perfectly under our command; and that the whole, or any part of it, may be excluded from, or admitted into, any part of the workings at pleasure.

It does not seem necessary to say any thing more in illustration of the general principles; but it is proper to notice certain circumstances which may occur to interrupt or derange their operation.

The passages of a mine are frequently obstructed, and sometimes entirely stopped up, by falls of stones, and other matters, from the roof; when this happens, an accumulation of fire-damp, more or less extensive, must unavoidably take place; but, in the mode of working and ventilating, which it is the object of this paper to explain, the limits of such accumulations are accurately fixed. Let it be supposed for instance, that a fall of the roof has occurred at K, so as to shut up the two passages *l*, *l*, below it. It is obvious, that the fire-damp will gradually collect, until these two passages are filled with it down the slope, as far as to the next openings below: but whenever they are filled so far, the accumulation must stop; because, as fast as new supplies of fire-damp arrive, they will proceed up the passages, *m*, *m*. It may be added, that the fire-damp, having so many other passages to escape by, would, in ordinary circumstances, collect very slowly below the fall at K, and thus afford a greater chance of its being discovered by the per-

* It is to be observed in fastening these doors at this point, that they are to open *down* the slope; and that the fastenings must be slight; merely sufficient to keep the door in its proper position, and to give way so as to allow the door to shut on the application of a slight degree of force. Their being accidentally shut cannot produce any bad effects, as no fire damp can accumulate either above or below them. The only inconvenience will be a temporary derangement of the atmospheric current.

sons appointed to explore the workings for this purpose, before the passages, *l, l,* were fully charged.

Such an accumulation of fire-damp, however, supposing both passages full, though very inconsiderable when compared with what frequently takes place in mines ventilated on different principles, would certainly be fatal to the miner who should heedlessly or unfortunately set it on fire. But it is probable that the mischief would not extend further. Having plenty of room to expand in every direction without meeting with resistance, it is certain that the force of such an explosion would be speedily diminished, and that, at a very short distance, it would be perfectly harmless. Its immediate effects would be confined to the blowing open a few of the slight doors which might be nearest to it, and which the return of the air would shut again by breaking the suspending string or fastening; and its ultimate effects would only amount to a temporary derangement of the atmospheric currents in the immediate neighbourhood.

The evolution of fire-damp in mines is in general gradual and slow: but not unfrequently great discharges of it are made into the workings, from what are called *blowers*. A blower is a fissure, or small opening, through which a stream of fire-damp rushes into the mine, in great quantity, and with considerable noise; as if the reservoir from which it comes were under the pressure of a head of water, which may in some instances be the case. Blowers are said in general to proceed from the roof, but are sometimes also observed to issue from the floor of the mine. The stream of gas issues from a blower in much less quantity after it has blown for some time than at the beginning; and is generally found to diminish gradually, until it ceases altogether.

When the discharge of fire-damp from a blower is so large as to be dangerous, there is no choice of the measures to be adopted; as there is only one which can be effectual, and that is, to lead the fire-damp the nearest way to the upcast shaft. This may be done in different modes, as may best suit the circumstances of the case. If, for instance, a dangerous blower issues in a place which is no common thoroughfare, as at N, the simplest mode of directing it to the upcast shaft, is to let it have the whole passage to itself, by shutting the lateral doors, *o, o,* and *p, p,* on each side, and leaving an opening in the lower one at R, to admit a sufficient supply of air to float the products of the blower to the upcast shaft.

But when a blower occurs in a part of the mine, through which it is absolutely necessary to pass, and that frequently, the mode of cure is more difficult, and consequently more expensive.

Let Fig. 3 represent a section of a passage, in which a blower, A, issues from the roof, and has formed an excavation in it. To carry off the fire-damp as fast as it issues, it will only be necessary to fix the tube, B, along the roof of the passage, so that its upright part, C, may open into the highest part of the excavation, which may be enlarged and deepened for this purpose. The other end of

the tube must open into the upcast shaft at D.* We have already seen, that, whatever kind of air may be contained in a similar tube, and in a similar position, it may be made to proceed upwards, by the application of heat. In this case, however, it will act of itself, but will, of course, be assisted by heat, which may be applied by surrounding a foot or two of it with boiling water. This tube may be made of any materials which may be supposed to answer best, as cast-iron, sheet-iron, wood, &c.; and may be made of any form, flat, square, or round. If made of thin deal, which will be cheapest, that part of it which will be immersed in hot water must of course be made of iron.

The occurrences of falls from the roof, and of blowers, are by far the most formidable of the obstacles which can happen to prevent or to hinder this system of ventilation from producing its full effects: and while your readers are estimating the probable efficacy of the means proposed to obviate them, it is proper to inform them, that there is scarcely any standard of comparison to measure it by; that these obstacles occur under the present system of ventilation in all their pernicious force, without having been obviated, or even successfully limited.

Many of your readers must, by this time, be desirous of knowing something of the mode of ventilation at present employed, for the purpose of comparing it with the system which is here proposed. And it happens most fortunately, that the *Annals of Philosophy* will furnish them with the means of so doing. At page 355, vol. i., of the *Annals of Philosophy*, they will find a most luminous description, (accompanied by a plate) of the workings of the Felling colliery, on the 25th of May, 1812, on which day the fire-damp exploded, hurrying, under most awful circumstances, no fewer than 92 persons into eternity. The terrific scene cannot be better described than in the words of the humane author. "When," says he, "the air has proceeded lazily for several days through a colliery, and an extensive magazine of fire-damp is ignited in the wastes, then the whole mine is instantly illuminated with the most brilliant lightning—the expanded fluid drives before it a roaring whirlwind of flaming air, which tears up every thing in its progress, scorching some of the miners to a cinder, burying others under enormous heaps of ruins shaken from the roof, and, thundering to the shafts, wastes its volcanic fury in a discharge of thick clouds of coal dust, stones, timber, and, not unfrequently, limbs of men and horses."†

On reference to this narrative, and to the plate by which it is illustrated, the mode of ventilation at present in use will be found correctly exhibited: and on comparing it with the proposed system,

* As it will not again be necessary to refer to the figures, it may be proper to say, that they are not constructed on any regular scale of proportion; being merely intended to elucidate the application of the general principles.

† Page 9 of the Narrative, and page 359 of the *Annals*.

it will be much easier to find the points wherein they differ than those in which they agree.

It will be perceived, in the first place, that the workings, instead of being oblique, are rectangular: and it will immediately occur, that on the face of a considerable slope, rectangular workings are almost of all other forms the most likely to detain a portion of the fire-damp in the lateral branches; while oblique workings are of all others the most likely to let it pass upwards. It will next be observed, that the great object under the present system is to convert the whole of the workings into a tube, which winds up and down the slope alternately: and which is terminated at one end by the downcast shaft, where the atmospheric air enters, and at the other by the upcast shaft, where it is discharged. This certainly wears an appearance of great simplicity, and could we but forget the declination of the strata, and the levity of carbureted hydrogen gas, it might be reckoned an admirable contrivance. But neither the Felling colliery, nor perhaps any other, is a dead level: on the contrary, it dips at the rate of* one yard in twelve, a declivity sufficient to give motion to a loaded waggon, which in its descent drags an empty one up the inclined plane, thereby saving the expence of many horses. Taking the mean length of these workings to be 500 yards, in the direction of the dip and rise, the astonishing fact meets us, that, whatever portion of fire-damp may be evolved in any one passage, it must, after having actually ascended 125 feet perpendicular, again *descend* as many feet; and this repeatedly, before it can arrive at the upcast shaft. What are the consequences to be expected from this preposterous opposition to the natural levity of fire-damp? they are these: that it will resist being carried down the slope, with all the powers of doing so with which nature has furnished it, which are by no means small; that, though a great part certainly may, and must descend, yet it will take all opportunities of absconding by the way into the lateral openings; that a part of it will always linger in the upper ends of the passages, and in many a corner whence it cannot be dislodged; that, in short, it will *accumulate* in the upper parts of the workings, and that it may be expected to explode there. And your readers will accordingly find, that the fire-damp in the Felling colliery actually exploded in the precise spot, where, from the influence and combination of the circumstances just enumerated, an explosion was to have been expected. Is another proof wanted? a melancholy one is at hand. Let your readers refer to an account of a second explosion in the *same* colliery, which is likewise recorded in the *Annals of Philosophy*, vol. iii. p. 132, and which cost the lives of 23 persons; and they will find that the fire-damp again

* P. 33 of the Narrative, and p 442, vol. i. of the *Annals*. To understand this part of Mr. Hodgson's narrative perfectly, it is necessary to substitute the word *south-east* for the word *south-west*, which must have been an error of the press, and which has been copied into the *Annals*.

exploded in the higher part of the workings. It would be cruel to attach blame to the persons concerned in managing the colliery where these fatal events occurred. We are informed by a person, whose veracity cannot be doubted, "that this mine was considered by the workmen a model of perfection in the purity of its air and orderly arrangements." And we are informed, that, at the time the second explosion took place, "the current of atmospheric air was so strong, that it was difficult for the workmen to keep their candles from being blown out by it;" if, however, under circumstances so favourable, and under careful management, such fearful accidents occur, the irresistible conviction flashes upon the mind, that the existing system of ventilation must be radically defective.

Your readers on a careful examination of the plan of the Felling colliery, will observe, that the workings are formed into a sort of *double* tube, which is called a *double air course*. They will likewise comprehend, that if a fall of the roof should shut up one of the branches of this tube near the upper end of the mine, the fire-damp must rapidly accumulate in the choaked passage, while the ventilation would proceed as usual through the other. Now, if we suppose this passage to be 500 yards in length from the dip to the rise, six feet wide, and six feet high, we shall have a magazine of fire-damp containing at least 2,000 cubic yards. So much for the *quantity* of fire-damp which it is possible may accumulate in mines conducted on the present system.

Having thus converted the workings of a coal-mine into a tube, whether single, double, or treble; the next object of solicitude, under the present system of ventilation, is, to strengthen every part of this tube as much as possible. For this purpose, the lateral openings are strongly and entirely stopped. In the Felling colliery we are told, "that the stoppings are made of brick and lime, and are further strengthened on each side with a wall of stone." Now, if a violent explosion of fire-damp should occur in any part of this strongly fortified tube, the inevitable consequences must be, that if the stoppings stand the shock, the destructive blast must visit every ramification of the workings before it can get to either of the shafts; and if the stoppings give way, that those persons, whom the force of the explosion could not therefore reach, must be cut off from the possibility of escape. "But this," says Mr. Hodgson, speaking of explosion, "though apparently the most terrible, is not the most destructive effect of these subterraneous thunderings. All the stoppings and trap-doors of the mine being blown down by the violence of the concussion, and the atmospheric current being for a short time entirely excluded from the workings, those that survived the discharge of the fire-damp are instantly suffocated by the *after-damp*, which immediately fills up the vacuum caused by the explosion." The existing system is therefore placed in a dilemma by this part of its arrangements, from which it will not be easily extricated. In the mean time, while the strength of stoppings is considered as an essential principle in ventilation, the miner must

keep his spirits up with the hopes (too often fallacious!) that explosions may not often occur, and that when they do, they may be trifling; well knowing, that if a sufficient quantity of fire-damp takes fire, he must be blown to pieces in the first instance, or suffocated in the second.

The results then of the comparison which has been thus far pursued are: 1. That the proposed system of ventilation affords to the fire-damp every facility of making its escape. 2. Having so many other channels to escape by, it will take a *considerable time* to collect, in a passage which may be accidentally choaked. 3. By limiting the fire-damp, which *can* possibly accumulate, to a fixed quantity, we necessarily restrict in the same proportion its destructive effects if it should explode. 4. By affording to this limited quantity of fire-damp a free passage in every direction when it accidentally inflames, we prevent it from extending its ravages to distant quarters of the mine. This part of the subject may be here concluded by observing, that these important advantages will be in vain sought for in examining the arrangements of the existing system of ventilation.

There are still some points, the discussion of which, from their intimate connexion with the subject, it would be inconsistent with the objects of this paper to omit. After having described the mode of ventilating the Felling colliery, Mr. Hodgson goes on to say, "From this explanation it will easily be perceived, that the purity and wholesomeness of a coal-mine has no reference to its depth." This assertion appears to have been made without due consideration. In the first place, it is not attempted to be supported by facts, but by inconclusive reasoning. In the next place, it is at variance with numerous facts: and though an attempt has been made by an anonymous writer in the Philosophical Magazine, to evade the application of these facts, this writer must "submit to be told," (as he phrases it,) that evidence of this kind can never be superseded by any species of reasoning, while the facts themselves are unassailed.

From the constitution of carbureted hydrogen gas, it is evident, that we cannot retain it at the surface of the earth, much less at any considerable depth below that surface, but by absolute force; hence this gas can never exist in a coal-mine, but in a state of compression, more or less forcible, from the moment it is evolved, until it is liberated by ascending into the atmosphere. But we know that atmospheric pressure increases as we descend below the surface of the earth, in the same proportion as it diminishes when we ascend above it. The degree of compression in which fire-damp is held below ground must, therefore, be regulated by the *depth* of the mine. But we also know, that compression is one of the principal conditions of explosion, and that the more forcible the compression, the more violent the explosion, and *vice versâ.* Another remarkable fact, but well established, is, "that the workings of a colliery are often inaccessible with candles near the

downcast pit, called the *first of the air*; while they may be safely entered with any description of lights near the upcast pit, called the *last of the air.** The first of the air is the most dense, and as fluids are known to propagate pressure equally in every direction, it communicates its own degree of density to the fire-damp, with which it may meet in the neighbourhood of the downcast pit, thus fulfilling one of the conditions of explosion. This density will diminish as the mixture rises to the upcast shaft; it will also be diminished by the increase of temperature which it will gradually acquire by the way, and the air will consequently be deprived in a corresponding degree of the property of exploding. Until these, and other facts, can be otherwise explained, it may be taken for granted, that the deepest part of a coal-mine, or that part of it where the inflammable mixture is more forcibly compressed, is the most dangerous, and that of any number of mines, of unequal depths, affording the same quantities of fire-damp, the deepest must be the most dangerous. This discussion may close here, with a quotation from a memoir by Grotthuss, which appeared in the Annales de Chimie, for April, 1812. It is peculiarly applicable, and may help to deter anonymous writers from hazarding unsupported opinions on the subject. Having, by the most satisfactory experiments ascertained the fact, that gaseous mixtures are more or less inflammable as the pressure upon them varies; "D'après cela," says he (page 41,) "il est très possible, qu'au fond des mines de sel à Cracovie, à Amsterdam, ou dans un autre ville basse, un mèlange gazeux pourrait être enflammé, tandis qu'il ne serait plus inflammable, dans une ville tres-elevée comme à Quito dans l'Amerique Meridionale."

There is yet another cause of compression, which must produce effects proportioned to its intensity upon every species of air which is found in coal-mines. This is the force, by which the current of air is induced, or accelerated, through the workings, from one end of a mine to the other; whether by exhaustion, or by the more common mode of rarefaction by the application of heat. The variations of atmospheric compression are wholly beyond our controul; but it seems probable, that the other species of force might be so regulated in its application, as to balance in some degree the effects of those variations. When the barometer sinks, vast quantitities of gas are liberated from every perpendicular fissure in the roof, in which it has been confined by the superior weight of the air.

Were the downcast shaft occasionally closed, a similar effect would follow, from the suspension of the force which induces the atmospheric current through the mine. A quantity of gas proportionate to the degree of force which held it in confinement, would be thus set at liberty, and would find its way to the upper

* See a letter by Mr. John Buddle, in the first Report of the Society for preventing Accidents in Coal Mines, page 20.

part of the workings. The shutting of the downcast shaft for one quarter of an hour would in this manner clear the mine from a very considerable quantity of gas. The exhausting pump ought never to be omitted at the upcast shaft; vast quantities of fire-damp might be extracted by it when the downcast shaft is shut; every stroke would extract a portion of the gas, and set another portion at liberty in the workings, ready to be swept out when the atmospheric current is again introduced. These operations, regularly performed, would go a great way in preventing the overwhelming discharges of fire-damp, which occur when the barometer suddenly sinks, after having been steady for some time, and which arise, not from an increased production or evolution of the gas, but from its sudden liberation from confinement. It ought also to be deeply impressed on the minds of viewers, and by them on the minds of workmen, to take care that no unnecessary holes or excavations be made in the roof of the mine. The collier ought to be made to understand, that even the mark of his pick in this part will contain, and retain, a portion of fire-damp, ready to expand into mischief at the first opportunity.

It appears from Mr. Dalton's experiments, that two gases, very considerably different in their specific gravities, as oxygen and hydrogen, will *mix* when confined together in a *close* vessel. It occurs to me that advantage might be taken of this fact in ventilation. A mine might be shut up for a few hours, until the mixture were completed. By then turning on the atmospheric current, the aerial contents of the workings would be hurried out before they had time to commix: which would have the effect of cleansing the mine of an enormous quantity of inflammable air. Why should not operations so simple, and so highly important, be executed on Sundays, while labour of other kinds is suspended?

From what has been said your readers will readily perceive that the power of complete ventilation has a certain and strongly marked limit. When, after every power of increasing the supply, or of accelerating the progress of atmospheric air, has been applied, the proportion of fire-damp discharged by the upcast shaft amounts to $\frac{1}{12}$th part of the whole quantity of air emitted by it; the ventilation of that mine must be incomplete, and the mine dangerous in the same degree. With due precaution, however, it may still continue to work, as allowance must be made for the effects of temperature and rarefaction; and because, being collected from distant quarters, the air at the bottom of the upcast shaft may be mixed to the dangerous point, while the other parts of the mine may be considerably below it. This, however, is a vast quantity of fire-damp,; more, certainly, than is constantly discharged by any pit in existence; and if accidents do nevertheless frequently occur, long before the whole quantity evolved in the mine amounts to $\frac{1}{12}$th part of what the shaft can discharge, they can only be referred to the imperfection of the system of ventilation pursued, in allowing the gas to accumulate, or by hindering it from making its escape as

fast as it is produced. It does not seem necessary to enter into any detailed account of the various contrivances in use for the purposes of producing, or quickening, a current of atmospheric air through the workings of coal-mines. It may be sufficient to say generally, that they are simple and efficacious, and cannot therefore admit of much improvement. Those who may wish for minute details on this point are referred to Mr. Buddle's letter in the Report already quoted, which contains much useful information, and which is so much the more valuable, as it is the only thing of the kind before the public. They will not, however, be able to discover from it, that any one part of a coal-mine is higher than another; or that fire-damp possesses any other property than that of inflammability. They may likewise be startled on hearing from Mr. Buddle, (page 21,) that he considers the existing system of ventilation "to have arrived at an admirable degree of perfection;" and that "on the strength of his own experience (page 22,) he freely hazards his opinion, that any further application of mechanical agency towards preventing accidents in coal-mines (which are infested with fire-damp in a high degree,) will be ineffectual." Mr. Buddle's experience ought to have guarded him against supposing further improvement impossible. When he himself has invented perhaps the greatest improvement in the system, viz. that of the *swing door*, which is both simple and ingenious, and which Mr. Buddle will perceive, if this should meet his eye, admits of a far more extensive application than he has made of it.

Your readers will find (page 21,) that "the improved system, (as it is called) was introduced into the collieries on the Tyne and Wear, about *the year* 1760, and has *ever since* continued in general use in collieries abounding with inflammable gas, *without any rival method being thought of*, or *any improvement*, except the mechanical auxiliaries detailed in the descriptions of sections three, four, five."*

This fact of itself will, in the opinion of many people, amount to a proof that there must now be room for improvement; this proof will be further strengthened by the recollection that almost all the knowledge we possess respecting gaseous fluids has been acquired *since* the year 1760. If any doubt should remain on this point, it will be entirely dissipated on finding that, notwithstanding "the admirable perfection to which the improved system has arrived," its arrangements have not the slightest reference to the well-known levity of carbureted hydrogen gas.

Thus, Sir, have I endeavoured to place before your readers, in the plainest manner I could think of, a general outline of what I conceive to be a material improvement in the mode of ventilating coal-mines. I am persuaded that ventilation has at last become a

* These auxiliaries are merely different modes of accelerating the atmospheric current through the workings, by exhaustion and rarefaction of the air. The apparatus for which are all *above ground*.

subject of general interest, and I should therefore hope that these sheets may at least contribute in some degree to facilitate the complete investigation of it, which there is reason to hope will soon take place.

I remain, dear Sir, yours most truly,

J. Menzies.

Article VIII.

Account of a Journey to the Summit of Adam's Peak in the Island of Ceylon.

We have pleasure in laying before our readers the following extract of a letter, describing a journey to the summit of Adam's Peak, recently performed by two officers:

Colombo, Nov. 1, 1815.

While we were in Saffregam we resolved to put in execution a project of which we had talked at Colombo, and before our return to visit Adam's Peak. This plan we have accomplished. Leaving Baddegeddera on the morning of the 6th, we gained the summit on the next day at half past two in the afternoon. Our first march from Baddegeddera was $5\frac{1}{2}$ miles of tolerable road, through a fine and interesting country, along the left banks of the Caltura river, to the royal village and extensive lawns of Gillemalley. From this place the king received his store of jaggry. There are about 250 inhabitants, who are well looking, and of a creditable appearance. Their houses are numerous and comfortable.

From Gillemalley at three o'clock, we set out for Palabatula, situated on the top of the Allehentune mountain, at the distance of $4\frac{1}{2}$ miles in a N. E. direction. The ascent is about $2\frac{1}{2}$ miles in length. Here is a small religious establishment, where the priests live who have the care of the holy impression of the foot on the Peak, and there is good shelter for travellers. We slept at this place, and soon after day-light next morning renewed our journey, accompanied by one of the priests as a guide. The road leads for a mile and a half over a very rugged and abrupt ascent to the N. E., up the Nulu Hilla, at the bottom of which, about a quarter of a mile from Palabatula, we crossed the Caltura river, and all the way up to the top of the hill, we heard it on our right hand running below. The next ascent is the Hourtilla Hilla, of three quarters of a mile, still more rugged and difficult than the former; the road at some places having an angle of full 50 degrees. We then ascended the Gonatilla Hilla, about half a mile, still more steep, and the air became cooler and clearer. The next stage is to Deabetme, rather more than a mile, and here is the summit of this mountain, the road up which is one continual rise of four miles, without any intervening descent, although the hill

has four names, and each division is marked by a white washed stone on the right side of the road. There is here a small Ambelam (a Cingalese rest house), and the ruins of a building erected by Eyheylapolle (the late Dessave of Saffregam.) The Adikars, and Dessaves were accustomed to be carried as far as this point when they visited the Peak, which opens to the view bearing E. by N. The road now extends in a N. E. direction four miles, over the hills of Durmaragah, Pedrotollagalla, Mallemalla, Kandura, and Andea Malle Hilla, and is excessively steep and difficult. From the latter the Peak itself rises about a mile or three quarters in perpendicular height; from this place the way is fair climbing, the direction at first N. E., then S. E., again N. E., and lastly, N. W., where the perpendicular ascent is encountered. This is only to be surmounted by the help of several massy iron chains, which are strongly fastened at top, let down the precipice, and again secured below; these chains are donations to the temple, and the name of the donor is engraved on one of the links made solid for that purpose. The height of the precipice is about 20 feet, and many holes are worn in the face of the rock by the feet of the numerous pilgrims who have ascended it with the assistance of the chains.

At half past two in the afternoon we reached the summit. It is an area of about one fifth of an acre, surrounded by a stone wall four feet and a half high, of four unequal sides, with two entrances, one on the south and another on the east, and an opening to the west in form of an embrasure. In the middle is a rock about nine feet high, on which is the famed impression of the holy foot. It has, in fact, a most shapeless appearance, bearing little resemblance to a human foot; and what is most unfortunate for the tradition of its being the last footstep of Buddha, when he strode from Ceylon to Ava, the toes, if they can be discerned, are turned towards the west. The clouds which arose as we were ascending prevented our having any view, and we occupied ourselves till four o'clock in taking a plan of the summit. We then found it was much too late to think of returning to Palebatula, and resolved to remain during the night on the Peak. I can hardly attempt to describe the extraordinary grandeur and variety of the scene that opened upon us at sun-set; above our heads the air was perfectly serene and clear, below a thick bed of clouds enveloped the mountain on all sides, and completely intercepted our view. But every now and then the beams of the sun broke through mass of clouds, and threw a brilliant light over the surrounding mountains, then suddenly the opening was closed, and all was again hid from our sight. These beautiful glimpses were often quite momentary, and frequently repeated, sometimes even twice in a minute, nor did the operation entirely cease until it was quite dark. We spent a wretched night in a most comfortless hut about 30 feet below the summit. There was a piercing wind, and the cold was far greater than I had ever felt since I left England; unluckily we had no

thermometer with us, but I think the quicksilver would not have risen above 40°.

The rising of the sun presented a magnificent scene, but quite different from that of the evening; the whole surrounding country, except Ouva, was covered with clouds, above which only the tops of a few mountains were visible. Hunas Garree Candy, bore 25′ N. E.; and a mountain that we decided to be Idalgasina, 22° S. E. The whole country of Ouva was exposed to view, and lay stretched out in appearance just beneath our feet. The sea on that side was perceptible, and bore S. E., which must have been in the neighbourhood of Paltoopane, and it was perhaps the Leway, or great natural Saltpan, that we observed.

At seven in the morning we began to descend the mountain, and reached Palabatula at noon.

Article IX.

Answer to Dr. Murray's Objection to Prevost's Theory of Radiant Heat. By Richard Davenport, Esq.

(To Dr. Thomson.)

SIR,

As you and Dr. Murray have done me the honour to discuss some arguments I had ventured to put together in explanation and confirmation of Mr. Prevost's theory of radiant heat, applying the theory to some phenomena that had been deemed inconsistent with it, I hope you will allow me room in your *Annals* for a brief explanatory reply to Dr. Murray's notice of my paper.

Let us suppose the metal canister of Dr. Murray's experiment to be set on a stand in the middle of a room, with the polished side facing the north, the blackened surface facing the south. (I at present suppose it empty, and of an equal temperature with the walls of the apartment.) Then set one thermometer on the north side of the canister, and near it: and another on the south side, at the same distance.

The canister intercepts from the north thermometer a large portion of radiant caloric emanating from the south wall of the apartment, and it intercepts from the south thermometer an equal quantity of that from the north wall. Neither of these thermometers will indicate any variation of temperature, although one surface of the canister radiate, and one does not.

Dr. Murray does not account for this circumstance. I account for it on Mr. Prevost's theory, by saying, that the polished surface reflects from the north wall as much as it intercepts from the south wall: and the blackened surface gives by its own radiation as much as it intercepts from the north wall. The thermometers

themselves lose heat equally by their own radiation, but receive equally their compensation.

Now, fill this canister with a freezing mixture: and I will take the liberty to put to Dr. Murray two questions.

1. Has not the blackened surface lost a part of its intensity of radiation?

This cannot be denied: for the theory supposes bodies to radiate in proportion to their temperatures.

2. Has the polished surface lost its power of reflection?

The answer must be in the negative, for the power of reflection does not vary with the temperature.

It follows then that unequal diminutions have been imposed on those powers of returning heat to the thermometer which before were equal. The radiating surface has lost more than the reflecting surface, and its thermometer receives less return than that on the reflecting side.

If this should appear to Dr. Murray, (as I cannot refrain from saying it does to me) to be a demonstration, I am sure he is far above an unwillingness to acknowledge it as such. If otherwise, I shall consider myself much obliged by his pointing out through the same channel such fallacy as he may detect in it.

I am, Sir, your very obedient servant,

RICHARD DAVENPORT.

March 12, 1816.

ARTICLE X.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

ON Thursday the 29th of February a paper by Mr. Ivory was read, containing an investigation of the theory of the attraction of capillary tubes. It has been long known that liquids rise in capillary tubes to heights which increase as the bore of the tube diminishes. This is universally ascribed to the attraction between the atoms of the tube and the fluid. Clairaut considered this attraction as extending to a sensible distance. But Newton, Brook Taylor, Hauksbee, Laplace, &c. were of opinion, that it is evanescent at sensible distances, and of course extends only over an extremely small sphere. Mr. Lesly, in a paper printed in the Philosophical Magazine for 1802, considered the effect of an attraction perpendicular to the surface of the fluid; and this also has been done by Laplace. As the thickness of the glass tube produces no effect upon the height to which the fluid rises in it, Mr. Ivory considers the opinion of Newton as established. He conceives, likewise, that his mode of investigating the subject has some advantages over that of Laplace; but as from the mathematical

nature of these investigations they could not be read to the Society, it is impossible to give any account of them.

At the same meeting a paper by Dr. Brewster, on the means of giving the property of polarising light to glass, and common salt, and fluor spar, by pressure, was begun. The explanation of the doubly refracting property possessed by several bodies, notwithstanding the many new facts ascertained concerning the polarization and depolarization of light, still continued as difficult as ever; but some of the late observations of Dr. Brewster promise to throw new light on the subject.

On Thursday the 7th of March Dr. Brewster's paper was continued. He showed that glass acquires the property of a crystallized body by being strongly pressed by means of a screw. A similar change is effected upon it by bending a plate of glass between the hands, the more strongly it is bent the greater is the effect which it produces on polarized light. He conceives, that in consequence of this property, new light will be thrown upon the effect of external pressure in crushing or altering the structure of bodies, as arches, &c.

On Thursday the 14th of March Dr. Brewster's paper was concluded. Fluor spar, common salt, and other singly refracting bodies, may by compression be made to acquire the properties of doubly refracting crystals; but upon calcareous spar, sulphate of lime, and other refracting bodies, no change is produced by compression. Animal jellies, by compression, or dilatation, acquire the same properties as doubly refracting bodies. The author conceives, that a very sensible dynanometer may be constructed by means of glass, which is one of the most elastic of all bodies. A number of glass parallelopipeds fixed together may be bent by weights suspended from their middle, and by the changes in their effect upon polarized light, will indicate the degree of bending which they have undergone. By enclosing glass in fusible metal he conceives that very minute changes of temperature will be indicated by alterations in the density of the glass. Glass surrounded by a hygrometric substance will also act as a hygrometer. Dr. Brewster considers double refraction as probably resulting from the action of a peculiar fluid, and stated some circumstances which appeared favourable to that opinion.

At the same meeting a paper by Mr. Babbage was announced, containing further observations on the theory of functions; but from the nature of the subject this paper could not be read.

On Thursday the 21st of March a paper by Sir Everard Home was read, on the mode of action of specific medicines. From experiments already made it is known that poisonous bodies, whether mineral or vegetable, do not produce their effects upon the body till they are introduced into the circulation: and the effect always follows whenever they are introduced into the circulation. Ipecacuanna injected into the jugular vein produces instant vomiting,

and opium immediate drowsiness. We know at present only two specific medicines; namely, mercury for the venereal disease, and the eau medicinale, which is a vinous infusion of colchicum autumnale, for the gout. It is well known that mercury produces its effects only when introduced into the circulation. The author gives an account of several experiments with the eau medicinale on himself and on dogs, which shows that it requires likewise to be introduced into the circulation before it produces its effects.

At the same meeting part of a paper by Dr. Thomson, on the composition of phosphoric acid, was read. Lavoisier first ascertained that this acid is a compound of phosphorus and oxygen. The result of his numerous experiments was, that this acid is a compound of two parts of phosphorus by weight, and three parts of oxygen; but there is reason for believing that he over-rated the proportion of oxygen, owing to circumstances mentioned in the paper. Rose made a set of experiments at a much later period, partly in the same way as Lavoisier had done, and partly by acidifying phosphorus by means of nitric acid. According to him, phosphoric acid is a compound of 100 phosphorus + 114·75 oxygen; but when his numbers are corrected by a more accurate analysis of phosphate of lead than he possessed, they are reduced to 100 phosphorus, and rather less than 100 oxygen: so that the proportion of oxygen which he found in the acid was too small.

On Thursday the 28th of March Dr. Thomson's paper was finished. The author made many experiments to determine the constituents of phosphoric acid, by acidifying phosphorus by means of nitric acid; but the results were unsatisfactory. He therefore had recourse to the method of Lavoisier. Small quantities of phosphorus (as one grain or $\frac{3}{4}$ of a grain) may be burnt in glass retorts by the heat of a lamp, without leaving any sensible residue. The mean of a variety of experiments made by the author in this way is that a grain of phosphorus, when converted into phosphoric acid by combustion, absorbs three cubic inches and two thirds of oxygen gas. From this result it follows that the acid is composed of

Phosphorus	100
Oxygen	123·46

To verify this result the author had recourse to the phosphate of lead, which is a compound of 2 atoms phosphoric acid + 1 atom yellow oxide of lead. He gives three analyses of this salt; one by Dr. Wollaston, one by Professor Berzelius, and one by himself. These analyses are as follows:

	Acid.		Base.
By Wollaston	100	+	370·72
Berzelius..	100	+	380·56
Thomson..............	100	+	398·49
Mean................	100	+	383·26

This mean, which corresponds very nearly with the analysis of Berzelius, is considered as exhibiting the true composition of phosphate of lead. From this the weight of an atom of phosphoric acid is shown to be 3·649. From experiments with iodine, &c. it is shown that an atom of phosphorus weighs more than one and less than two: from this it follows that phosphoric acid is a compound of 2 atoms oxygen + 1 atom phosphorus; or by weight of 2 oxygen + 1·649 phosphorus. According to this result phosphoric acid is composed of

Phosphorus	100
Oxygen	121·28

This does not differ much from the composition of phosphoric acid deduced from the combustion of phosphorus. A mean of both methods is taken, and the constituents of phosphoric acid are considered as

Phosphorus	100
Oxygen	123·37

This gives us 1·634 for the weight of an atom of phosphorus; 2·634 for the weight of an atom of phosphorous acid; and 3·634 for the weight of an atom of phosphoric acid.

The remainder of the paper is taken up with an account of the composition of the phosphates. The most remarkable of these are the combinations of phosphoric acid and lime. The author has ascertained the existence of no fewer than six salts composed of these two constituents. The following are the names and composition of these bodies:

	Atoms.		*Weights.*	
	Acid.	Lime.	Acid.	Lime.
1. Quadrosteo-phosphate	5 +	1	100 +	19·86
2. Binosteo-phosphate	5 +	2	100 +	39·73
3. Bige-phosphate	5 +	3	100 +	59·58
4. Osteo-phosphate, or earth of bones	5 +	4	100 +	79·47
5. Phosphate	5 +	5	100 +	99·33
6. Ge-phosphate, or apatite	5 +	6	100 +	119·16

The most important of these salts, and the one always formed in common circumstances, is the fourth. The second is obtained by dissolving osteo-phosphate in phosphoric acid. The first salt is procured when we decompose osteo-phosphate by sulphuric acid. That acid removes three fourths of the lime, and leaves one fourth united to all the phosphoric acid. It constitutes the glacial phosphoric acid of the shops, or the substance employed in the preparation of phosphorus. The third salt may be obtained by dissolving apatite in phosphoric acid. The fifth salt was formed by dissolving the requisite quantity of lime in muriatic acid, mixing it with the due proportion of phosphoric acid, evaporating the mixture to dryness, and exposing it to a red heat. The three first salts fuse before the blow-pipe into a transparent glass, tasteless, and insoluble

water. The three last salts are infusible. Apatite is found ready formed in the earth. Whenever a salt containing lime is decomposed by a phosphate, osteo-phosphate of lime is always obtained.

There are three combinations of phosphoric acid and potash, namely:

	Acid.		Base.
Phosphate composed of...........	1 atom	+	1 atom
Biphosphate	2	+	1
Subphosphate	1	+	2

There are two phosphates of soda.

	Acid.		Base.
Phosphate composed of	2 atoms	+	1 atom
Biphosphate	4	+	1

Ammonia resembles lime in its mode of combining with phosphoric acid.

The constituents of several other phosphates are given; but it would be tedious to detail the whole of them here.

LINNÆAN SOCIETY.

On Tuesday the 5th of March a paper by Mr. Robert Brown, Librarian to the Society, was read, giving an account of some anomalies in the structure of seeds. A paper on this subject by Mr. Brown had been read to the Society in 1813, and was at that time withdrawn, in order to obtain an additional number of facts. The present paper consisted in some new facts respecting the structure of naked seeds. According to the author, no seed exists without a covering; but sometimes this covering bursts before the seed comes to maturity. He gave a particular account of the structure of the seed of the leontice thalictroides, which had been mistaken by preceding carpologists.

On Tuesday the 19th of March a paper by Mr. Salisbury was read, on a natural family of plants, to which he gave the name of rodoraceæ. He divided it into three orders; namely, *andromedeæ*, *ericeæ*, and *epacrideæ*. The paper contained a detail of the different genera which constitute the order of andromedeæ. The author pointed out many new distinctions, which enabled him to subdivide several of the genera, and establish new genera.

GEOLOGICAL SOCIETY.

Jan. 5, 1816.—A communication from J. Taylor, Esq., M.G.S. on some remarkable appearances in coke, was read.

The coke in question is produced from two varieties of Newcastle coal, known in the market by the name of Tanfield Moor, and Pontop. The coal is charred in an oven of brick-work, of very simple construction, each charge being sufficient to cover the floor to the thickness of 18 or 20 inches; the combustion begins at the surface and proceeds gradually downwards. When all the bituminous matter has been driven off, the mouth of the oven is

opened, for the purpose of drawing the charge, at which period the coke presents the appearance of a glowing pavement, rifted into perpendicular columnar masses, the bases of which rest on the floor of the oven. Adherent to the sides of these rifts are occasionally found concretions, of a rather flat and small botryoidal external figure, of an iron black colour, and highly metallic lustre, resembling grey manganese, or black hematitic iron ore.

Intermixed with these are small arborescent tufts, about a quarter of an inch in length, adherent by their base to the mass of coke; each branch of which, when examined by the microscope, appears composed of minute botryoidal shoots.

A description of certain beds occurring above the chalk, especially of the plastic clay, by the Rev. William Buckland, M. G. S., was read.

The paper begins with a description of the beds, which occur at and about Reading, as exposed by various quarries. The lowest bed is the flinty chalk, immediately upon which rests a bed of sand, about eight feet in thickness; the lower part of which abounds in green particles, in rolled and angular flint pebbles, in small round teeth of fish, and in a species of oyster, commonly called the Reading oyster; the upper part of the bed contains a few green particles, but no rolled pebbles or organic remains. Immediately upon this bed rests a bed three feet thick of fuller's earth; above which occurs the plastic clay, in fine beds, more or less mixed with sand, for the most part of a dark red colour, and above 35 feet in thickness: these clays contain no organic remains or septaria. The next and highest stratum in the series is a loam 11 feet thick, becoming more clayey towards the bottom, and then containing ochreous concretions and balls of pyrites.

These beds occupy much of the ground between Reading and Newbury, and appear to lie between the chalk and the London clay; corresponding, therefore, in position, with the plastic clay of the Isle of Wight, and of Dorsetshire, as described by Mr. Webster; and with the plastic clay of the basin of Paris, which, according to MM. Cuvier and Brongniart, lies between the chalk and the calcaire grossier.

The green sand of Reading appears in many places near London, and south of the Thames, resting immediately upon the chalk, as at Woolwich, Lewisham, Charlton, &c.; but containing no organic remains.

Mr. Webster, in his valuable paper, "On the Formations above the Chalk," hesitates in what part of the series to place the shell beds of Woolwich; the determination of which question forms one of the important points in the present paper; and from many considerations, founded on personal inspection, and an authenticated list of strata, Mr. B. is inclined to consider them as occupying the middle part of the plastic clay formation, a formation which constitutes a real and important number of the great series, although

the beds of which it is composed exhibit great irregularities, both with regard to their mineral composition, and the presence or absence of organic remains.

Jan. 19.—A memorandum relative to the basaltic columns of the Isle of Salsette, by Mr. Babington, was read.

The Island of Salsette is separated at its northern extremity from the adjacent Mahratta coast by a narrow creek, on the eastern side of which runs a low ridge of basaltic hills, for the space of four or five miles. Wherever the rock is uncovered, are traces of the columnar structure; but in three places clusters of columns rise above the general surface, like so many bundles of reeds. The height of the most lofty columns is about 50 feet, the average diameter of each not exceeding 20 inches. The shafts vary in form from four to seven sided, and are not articulated. The rock is externally of a rusty brown colour, but internally is of a light bluish grey, with an irregular fracture, and not very compact.

The western hills of Bombay exhibit traces of the same formation; but the rock is much darker in colour, closer in its grain, of greater hardness and specific gravity.

A paper on the geology of the Lincolnshire wold, and the adjacent county, by Edward Bogg, Esq. was read.

If a line of section be drawn from Saltfleet, on the coast of Lincolnshire, through Louth and Wragby, to Lincoln, it will exhibit the following beds, proceeding from the newer to the older.

The country between Louth and the sea is flat and marshy, and presents alluvial clay mixed more or less with sand and marine organic remains. From Louth to the high hills in Donnington, a distance of about five miles, the country is occupied by the elevated district of the wolds, which consists of beds of chalk, the upper of which are of a white colour, and contain subordinate beds of flint, while the lower are of a reddish colour, and are destitute of flints. Immediately below the chalk is a bed from six to 10 yards thick, of coarse brown pebbly sand without organic remains. To this succeeds a bed, 12 or 14 yards thick, of clay with subordinate beds of lime-stone, the structure of this latter oolite, and it contains nodules of pyrites and bivalve shells. Below this lies a stratum of sand, of various colours, from dark brown to light grey, inclosing thin beds of sandy limestone with organic remains; the thickness of this stratum is considerable. The last of the series is a slaty clay, or shale, of unknown thickness; but which had been bored into for 100 yards, near the village of Donnington. It contains a multitude of beds of slaty clay, with marine remains, generally soft, but sometimes considerably indurated, and often very bituminous, of iron-stone and of grit. The surface of this last bed is covered in many places with alluvial deposits of blue clay and of grey marl.

ROYAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Royal Institute of France during the Year 1815.

Physical Department.—*By M. le Chevalier Cuvier, Perpetual Secretary.*

(*Continued from* p. 233.)

Mineralogy and Geology.

Among the questions which philosophers, occupied with the theory of the earth, usually agitate, there are few more difficult, or which have occasioned longer or more obstinate disputes than that on the origin of basalt and wacke, rocks which some consider as products of ancient volcanoes, while others view them as deposites from the general liquid in which the common rocks were formed, and as analogous to primitive trap.

M. Cordier, Divisionary Inspector of Mines, and Corresponding Member of the Class, having turned his attention to this problem, has contrived methods of resolving it, which are entirely new.

His first reflections enabled him to perceive that the greatest difficulty to compare matters of a disputed nature with those whose origin, whether volcanic or non-volcanic, is incontestable, depends upon this, that both are often composed of particles so mixed and reduced into a paste apparently so homogeneous, that it is impossible for the eye to distinguish them. Here chemistry cannot come to the aid of the senses, because it confounds all these particles in its analyses, and only gives as a result the list of their primitive elements, instead of distinguishing those which belong to each species.

M. Cordier, therefore, contrived a new mode of mechanical analysis, which consists in reducing to parcels the mineral species, the existence of which we have reason to suspect in the rocks which we wish to examine; to determine correctly the physical characters of these parcels, and their action under the blow-pipe; then to pulverise the rocks which we are examining, to separate by fanning or washing the different sorts of particles which this pulverisation has separated from each other, and to subject them to the same experiments as the different parcels of well known substances have undergone.

This is, as we see, a kind of microscopical mineralogy, of which M. Cordier has made an excellent use. The stony pastes, known to be lavas from historical proof, were readily subjected to this analysis. Their particles very easily separated. They presented but very few combinations, in which sometimes felspar predominated, sometimes augite, and in which titaniated iron was mixed in various proportions. With these three constant elements were mixed, but in a less general manner, hornblende, leucite, mica, olivine, and specular iron ore.

The basaltic pastes of a more or less disputed origin were divided with equal ease into their constituent parts, and these parts were not found different. All these ancient or modern pastes, whether considered as lavas or not, are then, according to the author, microscopic granites, in which the uniformity of the mixed tissue is only interrupted by small voids, somewhat more common in certain lavas than in others, and which appear to the naked eye homogeneous masses, in which predominate either the characters of augite or of felspar, and which therefore can only be distinguished into two kinds.

A part of the scoria which accompany the stony lavas, and which are the first products of the coagulation of the matters in fusion, are composed likewise of different grains, but finer, less regularly mixed, and yet of the same species as the masses which they cover. Another portion, more altered by the action of fire, approaches more to a vitrified state. Others are completely in that state; but still sufficient traces of their origin remain to enable us to recognise it. They always belong to one of the two principal orders of combination observed among stony lavas.

M. Cordier endeavours to explain, by the different state of the scoria, a phenomenon which has struck several travellers, that some currents of lava remain always sterile, while others are speedily covered with the finest vegetation. It is because the former, being more vitrified than the latter, are less easily decomposed.

The author examines likewise the obsidians, or volcanic glass; and comparing all the shades of their greater or less vitrification, he always finds traces of augite or felspar the principles which predominate in the two orders of lavas; and the obsidians fused into a black glass have shown perfect transitions to the most dense basalt. In a word, obsidian, scoriæ, lavas, basalts, do not differ in composition, but only in the peculiarities of their texture. Even in volcanic sand and ashes we may find, by washing, the same materials whose aggregation forms the neighbouring lavas. M. Cordier has followed these materials in the different substances after they have been altered by time, and has disengaged from them the new substances which have made their appearance in them, or which have filtered into their vacuities. He has not neglected the examination of any modification of these volcanic products, whether true or disputed, and he has every where found these general laws to hold; but when we pass to the ancient trap rocks, to which it has been attempted to refer the basalts, none of these marked characters can be perceived which establish such undoubted relations between lavas and basalts.

The mass of these ancient rocks has no apparent voids; we scarcely perceive grains in them, and they do not differ from each other in colour. They cannot be divided into separate parts; no mechanical analysis can be made of them. Consequently if a part of these rocks is composed of heterogeneous materials, it is impos-

sible to determine the mineral species to which these materials belong.

Their chemical analysis is likewise different, particularly because they contain no titaniated iron.

Hence the pretended analogy between the traps and the basalts is not confirmed by a rigorous examination.

As to the origin of the lavas, and the causes of their fusion, M Cordier does not venture to conjecture; but considering their mass as coagulated by an instantaneous crystallization, he easily resolves the problem so long disputed, whether the crystals in lavas have been carried quite formed from the bowels of the earth, and enveloped in them, or have been afterwards formed in their cavities, or have crystallized at the same instant that the rest of the mass consolidated. It is easy to see that he adopts the last of these alternatives.

He terminates this great and fine undertaking by a methodical enumeration of the basalts, and the products of volcanoes, arranged according to their materials of aggregation, and under the two predominating principles, felspar and augite.

This nature so mysterious of volcanoes, these immense chimneys of heat, under circumstances quite different from those which support fires at the surface of the earth, will long constitute one of the great objects of the curiosity of philosophers, and will excite their efforts as long as there remain any hopes of success. A young mineralogist, both zealous and well-informed, M. Mesnard de la Groye (of Angers), having had an opportunity in 1812 and 1813 to observe several of the phenomena of Vesuvius, has drawn up a journal of them, remarkable for its exactness, and mixed with many original ideas and conjectures.

Since the enormous diminution which the cone of the volcano underwent in 1794, when it sank more than 400 feet, all the eruptions have taken place from the summit, which seems to have prevented them from being so abundant and destructive as those which proceeded from the sides. The bottom of the crater has risen, and it is not impossible but it is filling up. Hence M. de la Groye draws as a conclusion that we must not always refuse to allow a mountain to be volcanic because it has no crater.

The flow of lavas is the less abundant the greater the quantity of scoriæ and lapillæ thrown out of the volcano during an eruption. The whole cone is covered with these little stones, which are soon altered by the acid vapours, and assume those lively and variegated colours which give them at a distance the appearance of turf in blossom, and which have led naturalists into the opinion that the crater is filled with sulphur. This is so far from being true, that it is seldom we even perceive sulphureous vapours. On the contrary, continued exhalations of muriatic acid are perceptible, and concretions of common salt are every where to be seen.

M. Mesnard de la Groye from this divides volcanoes into two

classes; those in which sulphur acts a conspicuous part, and those in which muriatic acid predominates. He places Vesuvius among the latter.

He points out, likewise, the constant smoke which rises from currents of lava, and which shows that it contains much moisture; for these smokes are entirely aqueous. No flames appear; but the red-hot sand and stones, and the reverberation of the internal focus on the vapours which issue out, occasion the appearance of them. Lava flows slowly; its sides cooled down form a canal for it, and keep it elevated above the soil, all covered with scoriæ. We know, likewise, that its heat is not equal to that of melted glass; for when it incloses the trunks of trees, it does not char them to the centre. M. de la Groye, therefore, believes that the lava owes its fluidity to some principle consumed by the fusion, and that this is the cause of the difficulty of again fusing lava which has become solid. The part of the mass which is not swelled into scoriæ has an aspect quite stony. To it the Germans give the name of *graustein.* The author compares the periods of the fusion of lavas to those through which those salts pass that melt after swelling up. He mentions curious facts respecting the prodigious duration of their heat, and concludes from them that they contain within themselves the principle of heating, and do not merely possess communicated heat. To all these remarks M. de la Groye adds a minute account of the great eruption of 1813, which produced an infinite quantity of lapillæ and ashes, but the lavas of which did not reach the cultivated country.

After having studied a burning volcano with so much care, M. de la Groye wished to ascertain the motives for arranging different mountains among extinct volcanoes. He accordingly visited one which M. de Saussure, and other great geologists, had placed in that class, but in which the obstinate Neptunists still found numerous pretexts to support their doubts.

This is the mountain Beaulieu, about three leagues from Aix, in Provence. The inequalities of the surrounding country exhibit marks comparable to currents of lava. Its extent is about 1200 fathoms in length, and 600 or 700 in breadth; its height about 200 above the sea. It is surrounded to an indefinite distance by lime-stone. Towards the east are the basaltic precipices which appear to constitute the centre of the whole system; but even in the basaltic part itself sea shells occur, and a great deal of limestone. The amygdaloids and basalts are covered with it in several parts. In others fragments of it are mixed, and constitute a kind of breccia. It has often penetrated the cavities of the amygdaloid.

The principal rock is the floetz green-stone of the Germans, composed of felspar and augite, sometimes in such large grains that it resembles granite. It forms a long ridge, and we pass from that rock by intermediate ones, comparable to trap properly so called, to common basalt containing frequently olivine, and of which Saussure observed some parts divided into prisms. There is, likewise, wacke,

which constitutes the basis of the amygdaloid, and which, when its cavities are empty, resembles exactly a porous lava; but which are most commonly filled with calcareous spar, like the mandelstein of the Germans. We find, likewise, a basaltic tuff filled with small calcareous pebbles, and containing augite, olivine, mica, and the other minerals so common in lavas. M. Mesnard saw at Beaulieu even a hollow which he considered as the remains of a crater. The author, after some general reasons against the objections of the Neptunists, concludes that this mountain was produced by a submarine eruption, and that the sea in which it took place continued long after to deposit lime-stone. Saussure has already expressed himself in favour of that opinion. Faujas has considered it as incontestible; and M. Mesnard thinks he sees in it a method of conciliating all the opinions about these pretended secondary traps so long the subject of debates.

Among the numerous remains of unknown animals which fill the strata of the earth, there occur marks of an animal of a singular form, composed of a kind of corselet, and of an abdomen formed of different segments, each of which is divided into three lobes. Naturalists have given them the name of *entomolites* and *trilobites*; but they have not sufficiently distinguished them from each other, and did not attempt to determine to what particular bed each belonged.

M. Brongniart, Director of the Manufactory at Sevre, which the Class has lately acquired as a member for the section of mineralogy, in place of the late M. Desmarets, has presented a paper on this subject; in which, after a careful comparison of the specimens that he procured, and likewise of the descriptions and figures left by preceding authors, he shows that there exist at least seven species of these trilobites; that their principal forms are sufficiently different to enable us to divide them into four genera, all of which must be arranged in the class of *crustacea*, and in the order in which the branchiæ are uncovered. The greater number of these trilobites belong to the most ancient, that is, to the deepest, beds which contain animal remains. They must, therefore, have been among the first living beings; and in fact as we approach the surface we find crustacea more similar to those which the sea nourishes at present; but the trilobites disappear entirely.

M. Gillet-Laumont, Member of the Council of Mines, and Correspondent of the Institute, has exhibited agates containing white circles distributed in a quincunx, which resembled some petrifaction of a polypus; but they were produced artificially. M. Laumont, who had observed that blows struck in a particular manner detached very regular cones from sand-stone (*grès*), applied the same method to agates, and produced in the same way conical fissures, the sections of which presented circles precisely similar to those exhibited at first.

M. Cordier has published a memoir on the coal-mines of France, and on the progress made in working them during the last 25 years.

He has shown that in this interval the produce has increased more than fourfold. This work, very important for the administration, is accompanied by a very interesting chart, exhibiting the extent of our coal districts, the principal mines at present working, and the direction of their different levels. It has been inserted in the Journal des Mines.

A shower of stones has fallen this year likewise in the neighbourhood of Langres, with all the usual circumstances. M. Pistollet, physician in that town, has collected some of them. They are quite similar to other stones of the same origin, except that their fracture is perhaps a little whiter.

M. Vauquelin, who had been charged last year with the examination of the aerolites of Agen, has presented some reflections on the state in which the principal elements occur in this sort of stones. A portion of the silica appears to him to be in combination with the magnesia. Sulphur is present united to iron; for the mineral gives sulphureted hydrogen, when dissolved in the acids. As to the chromium, it appears in a separate state, and sometimes in particles so large as to destroy all idea of combination.

(*To be continued.*)

Article XI.

Scientific Intelligence; and Notices of Subjects connected with Science.

I. *Flexible Sand-stone.*

Some fishermen, upon the east coast of China, near the province of Chincheu, brought up a considerable piece of stone in one of their nets. Observing something peculiar in its appearance, instead of throwing it overboard, they carried it to Canton to tempt the curiosity of the Europeans in that place. It was purchased by Mr. Garratt, the purser of an East Indiaman, who supposed from its appearance that it was a petrified buttock of beef, and on that account gave a considerable sum of money for it. It is at present in the possession of Mr. Mawe, mineral dealer in the Strand, where I had an opportunity of examining it.

It is an irregular shaped mass, weighing, when dry, 23 pounds four ounces avoirdupois; and when soaked in water, 24 pounds four ounces. It has certainly some faint resemblance in shape to a buttock of beef. One part answering to the bone and another to the flesh. But this appearance is owing to an external coating of ochre, and does not hold internally. On one side of it there is a coating of small quartz crystals. The stone is full of small round holes, placed at unequal distances, and not penetrating deep.

Colour partly snow white, partly wine yellow.

Lustre, dull. Fracture, fine grained, even. Fragments irregular.

It is probably composed of grains; but they are so small as scarcely to be distinguished by the naked eye. Opake.

Soft, or at least easily scratched with a knife; but scarcely reducible to powder between the fingers. Under the pestle of an agate mortar it is easily crushed to the finest powder; so that the grains of which it is composed are exceedingly small.

Specific gravity 1·6825.

The remarkable property by which this stone is distinguished is, that when steeped in water it becomes exceedingly flexible in every direction; but without the least elasticity. When allowed to dry it loses in a great measure this flexibility. Thus it differs from the flexible sand-stone ot South America, both in its external properties, and in the effect of water upon it.

To ascertain the constituents of this stone I fused 20 grains of it, previously exposed to a red heat, with carbonate of soda. The fusion was complete, and very easily accomplished, showing that the stone consisted chiefly of silica. The fused mass was dissolved in water, supersaturated with muriatic acid, and evaporated to dryness. The dry mass being washed in water, and thrown upon the filtre, left the silica. The liquid was precipitated by carbonate of soda, and the precipitate boiled in pure potash. Part was dissolved. The remainder was grey, dissolved with effervescence in muriatic acid, and formed selenite with sulphuric acid; it was therefore carbonate of lime, mixed with a very little iron. The selenite weighed $1\frac{1}{2}$ grain, indicating 0·62 grain of lime. The portion dissolved by the potash was alumina, and being thrown down by sal ammoniac, and washed and dried, it weighed 0·3 grain. Hence the stone was composed of

Silica	19·08
Lime with a trace of iron	0·62
Alumina	0·30
	20

II. *Curious Growth of a Plane Tree.*

Among the ruins of the old monastery of the New Abbey, in Galloway, there is a plane-tree, about 20 feet high, which grows on the top of a wall built with stone and lime. Being straitened for nourishment in this situation, many years ago it shot forth roots into the open air. These neither died nor drew back, but descended by the side of the wall, which is 10 feet high. It was several years before they reached the ground, during which time they conveyed no nutriment to the tree, but were supported by it. At length they dipped into the earth, and have since enabled the tree to grow with vigour. Between the top of the wall and the surface of the earth they have never thrown out either branches or

leaves, but have coalesced into a sort of trunk 10 feet high, and pretty thick, which is very singular in being now terminated by roots both at top and bottom.—This curious fact is related in a letter by Dr. Walker, late Professor of Natural History in the University of Edinburgh, to Lord Kames, dated Feb. 18, 1773, published in the Memoirs of the Life and Writings of Lord Kames by Lord Woodhouslee, vol. iii. p. 207.

III. *Depth at which Barley will grow.*

Dr. Walker, in the same letter, states, from his own observation, that barley will rise though sown to the depth of 10 inches, but will not rise if placed 12 inches deep. This depending upon the access of air to the grain, shows us how far under the surface of the earth the air is capable of penetrating.

IV. *On burning Clay as a Manure.*

(To Dr. Thomson.)

DEAR SIR, *Dumfries, Feb.* 19, 1816.

Being one of the constant readers of your *Annals of Philosophy*, and looking at your title-page, I see you give all agricultural subjects an insertion in your publication. Situated as I am, in a part of Scotland where agriculture is quite in its infancy, and where the application of chemistry to the improvement of that most useful branch of national industry is altogether unknown, I take the liberty of submitting the following observations to your consideration, hoping your kindness to an ignoramus may overlook any impertinent questions I am now about to put. All this country from Port Patrick to almost the eastern extremity of Scotland, is undergoing the operation of clay-burning. It is natural to ask what is the cause of all this, and what benefit, or what alteration, does the clayey mass undergo by this torrefaction by fire? Many times have I inquired from some of these great clay-burners, but could never obtain any explanation, as unfortunately none of these Gentlemen are at all acquainted with the science of chemistry. I can understand so far, that where the clay to be burnt contains any portion of calcareous matter in it, the operation of the fire may convert it into a caustic state, and by that means convert it into an active manure; also the carbonaceous matter used in firing the clay may, by having its ashes mixed with the burnt clay, be useful as a compost for various plants, as it has been ascertained that carbon is the food of plants. Some clay, also, may contain much sulphate of iron, which may sometimes, by an excess of acid, be injurious to vegetation; and the carbonaceous matter may, when used in burning the clay, tend to dispel the sulphuric acid, and leave behind the iron, which may absorb oxygen, and afford it in quantity to be absorbed by the roots of plants. I believe that iron is capable of absorbing various doses of oxygen, but I do not know whether it has been experimentally tried if the roots of plants can absorb the oxygen from the metal. I have heard some assert that oxide of

manganese has the same property of absorbing various doses of oxygen; but have not yet heard whether it be capable of furnishing it to the roots of plants. Sir H. Davy, I think, says that oxide of iron is very useful in vegetation; but of oxide of manganese he has not said a word. Should you think it worth your while, you will much oblige a constant reader, and numerous others who are engaged in clay-burning, by explaining what you conceive to be the benefit derived from this process. I have seen some fields having had the whole surface ploughed up, being a clay soil, and a good deal of vegetable remains mixed with manure, subjected to this wasteful torrefaction. Manure in Scotland is difficult to be had, and the management of it not at all understood. Surely this method of destroying it in the soil is most wasteful, and will finally prove highly injurious to the interests of agriculture, if not soon checked. If at any time you should find leisure, some papers on the management and preparation of manure on sound chemical principles, introduced into your publication, would be well received by your northern friends. With hopes of soon seeing your attention given to the explanation, and what you conceive may be the advantages of clay-burning,

I remain, dear Sir,

A Constant Reader.

I am sorry that I am quite unable to give a satisfactory explanation of the practice alluded to in the preceding letter. On reading Mr. Craig's letter on the subject, when in Edinburgh last year, I made some inquiry about it, and was informed (I think) by Professor Jameson that he had seen some specimens of the clay thus burnt, and that in reality it was a *marl.* This explanation satisfied me at the time; but it is quite obvious, if the sub-soil from Port Patrick to Berwick be treated in this manner, that its nature must vary greatly in different places.

The burning of clay as a manure is not a new discovery, as it is supposed to be, in the Farmer's Magazine. Dr. Reid, in a letter to Lord Kames, written in 1775, says, "If wet clay is put into the fire uncompressed, I am informed that it burns to ashes, which make no bad manure." (Life of Lord Kames, vol. iii. p. 224.) We can assign some conjectural advantages that may result from burning clay, and laying it as a manure on clay land. From the curious table by Dr. Schübler, inserted in the last number of the *Annals* (p. 208), we see how very adhesive (or *heavy*, as agriculturists term it) clay soil is, and how strongly it retains moisture. Now when clay is burnt it loses these qualities. Such a mixture, therefore, may tend to render a clay soil less adhesive, and less retentive of moisture; but if this be its only use, fine sand or calcareous sand would probably answer better.

From Mr. Craig's description of the burning, we may infer that the heat applied is very small; for he says that the combustion is not apparent, unless you open up the heap; and that in such a case

it goes out, and cannot be again kindled. Is it not, therefore, possible that this apparent burning of the sub-soil is in fact only accomplishing imperfectly what Mr. Vanderstraeten says is performed every third or fourth year in Flanders. In that country, according to him, every third or fourth year the sub-soil, which is soaked with the manure that has been laid on the surface, is brought to the surface by trenching the whole ground, while that which was formerly the surface is converted into sub-soil. By this method he says the necessity of fallowing is saved; for the sub-soil in reality lies fallow for three successive years.

In case our farmers should be tempted to try this experiment, it may be proper to mention that the Waes, or the country between Antwerp and Ghent, in which Flemish farming has been carried to the highest point of perfection, was originally nothing but sea sand, and that it has become the rich, light soil, which we find it at present, from the great quantity of manure laid upon it, for a period of at least three centuries.

V. *Lamp for Coal-Mines.*

(To Dr. Thomson.)

Pereant qui ante nos nostra invenerint aut dixerint.

SIR,

I again prefix my motto by way of correcting the press of the last month. The figure of the lamp is at once an answer to the objection in The Philosophical Magazine, that the tube may receive explodible air from a blower, whether the lantern be moving or stationary. The safety principle of Sir H. Davy's lantern was at first supposed to be foul air within the lantern derived from the combustion of the wick. This was an error; and the diminished flame has been shown to be owing to the diminished supply of air. At first air was admitted by one or two large holes at the bottom of the lantern; explosions took place; to these holes, diminished in size, and increased in number, tubes were added. Finally, the holes were multiplied until they deviated into gauze. At every progressive change The Philosophical Magazine abused those who doubted. If I am now asked if I trust in the gauze, my reason for doubting is the abuse which The Philosophical Magazine continues to bestow on all who shall doubt. You have not developed the principle upon which the benefits of the gauze depend. You talk of a fixedness of the air, which cannot be. If an explosion takes place, without any considerable extrication of heat, the contact of the adjacent wires cools down the red-hot air, and renders it incapable of kindling combustion without. This indicates an adaptation of apertures and wires, which will vary with the nature of the explodible air and the heat developed. There will be, then, no safety but what depends upon this due arrangement, and the access of explodible mixtures not yielding more heat in combustion than is provided. If this heat be in excess, if a stitch be dropped

in the gauze, if the gauze be burst by the explosion, all is lost. Will a bomb inclosed in metallic gauze explode in a magazine without firing it?

To Sir H. Davy all credit is due for the perseverance with which he has continued to review, and endeavoured to improve, the first suggestions of his mind, and for the benevolent purposes by which his labours have been actuated.

Complete success seems not to be attainable in any of the ways attempted, and security should rather be expected from the change of system which has been before recommended.

VI. *Results of Hygrometrical Observations made in the Basin of the Atlantic Ocean.*

Period of the observations.	Latitude.		Data.		Quant. of vapour contained in the air.		Quantity of water evaporated in an hour's time.	
			Centigrade therm.	Hygrom.	To saturation.	In reality.	The air being dry.	In reality.
1799.					(*a*)Gram.	(*b*)Gram.	(*c*) Millimetre.	(*d*)Millimetre.
June 9	39°	10′	14·5°	82°	14·6°	11·4°	0·53°	0·13°
15	30	36	20·0	85·7	20·0	16·2	0·74	0·14
16	29	18	20·0	83·8	20·0	15·7	0·74	0·16
30	18	53	21·2	81·5	21·3	16·0	0·79	0·20
July 4	16	19	22·5	88	22·9	19·4	0·85	0·13
10	12	34	24·0	89	24·8	21·5	0·93	0·13
12	10	46	25·4	90	26·7	23·5	1·01	0·12
14	11	1	25·0	92	26·8	23·8	0 98	0·09

Humboldt's Personal Narrative, vol. ii. p. 90.

VII. *Curious Cause of Headach.*

A gardener's wife at Vienna was, at the age of 24 years, seized by a violent headach, which continued for several years, and drove her almost to despair. She was at last advised to take snuff as a remedy, in order to promote a discharge of mucus. Happening to have some assafœtida in the house, she mixed it with the snuff, on the supposition that it might increase the effect. The consequence was, that a worm was discharged from the nostril similar in appearance to the common grub. This circumstance induced her to continue to use the mixture of assafœtida and snuff. Eight more worms were discharged. In short, by the use of the remedy, 48 worms in all were discharged, and the headach was completely removed. Dr. Frank, who relates the case, supposes that the worms had been lodged in the frontal sinus.

VIII. *Strength of Iron.*

An experiment was lately made at Blackwall to determine the strength of the iron employed in the manufacture of iron cables. It was found that an iron wire $1\frac{1}{4}$ inch in diameter was broken by a weight of 40 tons; therefore an iron wire of one inch in diameter

would be broken by a weight of 25·6 tons. This is considerably less than the strength of iron resulting from Sickingen's experiments. He found that an iron wire 0·078 of an inch in diameter was broken by a weight of 549·25 lbs. avoirdupois. Now if we suppose the strength of iron wire to vary as the squares of its diameter, the preceding experiment will give us the weight capable of breaking an iron wire of the diameter 0·078 of an inch 348·88 lbs. avoirdupois. Hence, supposing both experiments correct, English iron must be materially weaker than Swedish iron.

IX. *New Ore of Copper.*

Mr. Mawe, mineral dealer in the Strand, received some time ago a cargo of minerals from Cadiz, which had been shipped originally at Vera Cruz, and of course are the produce of Mexico. Among them there were a few specimens of a copper ore, which, to me at least, was new; nor have I been able to find any allusion to it in any of the mineralogical books which I have consulted. The specimens which I have seen being few and small, the following description will be imperfect:—

Colour, verdigris-green, with a tint of blue. The central parts of the distinct concretions appear darker coloured than towards the circumference, owing, I conceive, to the translucency of the edges of these concretions. Some few of the concretions are coated with a whitish crust, but this is uncommon.

Most of the specimens had a botryoidal form; one of them was thin, as if it had constituted a portion of a thin vein.

Lustre, vitreous, and varying much in intensity. The internal lustre of a distinct concretion fully as strong as that of glass; but the external lustre is often dull.

Fracture compact, conchoidal. Fragments somewhat rounded with blunt edges. In granular distinct concretions rather larger each than a grain of mustard seed. Translucent on the edges. Nearly as hard as calcareous spar. It is readily scratched by a knife. The distinct concretions are very easily separated from each other; but a single concretion is not more easily frangible than alum or rock salt. Brittle. Specific gravity 2·238.

When a distinct concretion is thrown into nitric acid, no effervescence takes place; but if we reduce the mineral to powder, it effervesces in that acid, and is partly dissolved. The same solution takes place in the course of a few days if a lump of the mineral be put into nitric acid. A white insoluble powder remains, which fuses into a glass with potash, and is therefore silica. The nitric acid solution is blue, but becomes green when mixed with muriatic acid. 24·1 grains of the mineral treated in this way furnished 6·1 grains of silica; and the copper, being thrown down by a plate of zinc, weighed 10·5 grains. Now 10·5 grains of copper constitute 13·125 grains of peroxide of copper. This oxide in the ore was united to carbonic acid and carbonate of copper, as I have ascertained by experiment, and is composed of one atom acid and one atom

oxide. An atom of carbonic acid weighs 2·751; and an atom of peroxide of copper, 10. Therefore the carbonate of copper in the ore amounted to 16·736 grains. Hence the ore is composed of

Carbonate of copper	16·736	69·44
Silica	6·1	25·31
	22·836	94·75
Loss	1·264	5·25
	24·1	100·

I had not a sufficient supply of the ore to enable me to decide whether this $5\frac{1}{4}$ per cent. of loss be owing to the presence of water, or to an error in my mode of experimenting. I think it not unlikely that I may have lost a little of the copper, as I washed it repeatedly with water on a watch-glass, and poured the water off in order to get rid of the whole of the zinc solution. Now it is possible that a flock or two of the copper might have been carried off by the water without my perceiving it. The loss of half a grain of copper in that way would account for the greatest part of the loss. At the same time I am not aware of having lost any copper, and Klaproth found six per cent. of water in a specimen of blue carbonate of copper which he examined.

X. *Death of Guyton de Morveau.*

Some months ago M. Guyton de Morveau died in Paris, at a very advanced age. He was probably the oldest chemist in France, having been known as a writer for more than 40 years. His Digressions Academiques, the earliest of his writings with which I am acquainted, was published in 1773. As an experimenter, his merit was not very considerable; but he possessed a much more accurate knowledge of the history of the science than most other French writers. Several of the articles in the first volume of the chemical part of the Encyclopædie Methodique, which he wrote, are excellent, especially the article *Acid.* It was one of the earliest chemical tracts that I read, and I was indebted to it for a great deal of valuable information. There is a striking contrast between the volume which Morveau wrote of that work, and the various volumes which were contributed by Fourcroy, and the superiority is entirely on the side of Morveau. His reputation was much higher about the year 1787 than some years after. I ascribe this, in a great measure, to the praises lavished upon him by Mr. Kirwan. I shall take a future opportunity of giving a more particular account of the writings of this chemist, and of stating the chemical facts for which we are indebted to him.

XI. *Indian Arrow Root.*

This very agreeable starch is obtained from the roots of the *maranta arundinacea*, an American plant, which has been long cultivated in the West Indian islands, and which is said to grow wild in Jamaica. Tussac, in his Flore des Antilles, published in 1808,

gives it the name of *maranta indica*, and says that it came originally from the East Indies; and Bernhardi, Professor at Erfort, says that the maranta arundinacea and maranta indica are two distinct species; but both these statements are erroneous. The plant is not known in the East Indies; and the fecula known by the name of Indian arrow-root is extracted from the plant called maranta arundinacea by Plumier.

This plant grows to the height of about three feet, and dies down to the root every year. The roots are about $1\frac{1}{2}$ inch thick, covered with scales. These roots are washed clean, and pounded in a mortar. The powder is well washed, and the woody fibres separated from it. What remains is the starchy part. It has a beautiful white colour, and makes an exceedingly pleasant article of food when properly dressed. I have been assured that the article usually sold in London under the name of Indian arrow root consists chiefly of potatoe starch.

XII. *Identity of Galvanism and the Nervous Influence Vindicated.* By Dr. Wilson Philip.

(To Dr. Thomson.)

SIR, *Worcester, March* 5, 1816.

In the account in the *Annals of Philosophy* of that part of a paper of mine which was read before the Royal Society on the 25th of last month, it is observed that I go rather further than my experiments will warrant when I conclude that the nervous influence and galvanism are the same. It is clear, it is observed, that the section of the nerve interrupts the nervous influence, and that my experiments, supposing them correct, show that galvanism puts an end to this interruption, but that it may do this merely by serving as a conductor to the nervous influence. It is impossible to receive a perfectly correct idea of all parts of a paper of this kind from hearing it once read. From the way in which the experiment was made, there seems to be no room for the explanation here pointed out; for the cut ends of the nerve were so far from being applied, or even made to approach each other, that the upper portion was wholly neglected, and the lower portion drawn out, and coated with the tin foil.

It has been remarked that this experiment should have been made on some other animal than the rabbit. This suggestion comes from a quarter of such authority in physiological questions, that I have felt myself called upon to attend to it. I have repeated the experiment on dogs, and found the result, both with regard to the stomach and lungs, in all respects the same as in rabbits. The galvanism was not applied in such force as to occasion any expression of pain, which it does if the power of the trough is more than occasions a slight twitching in the fore legs. From these experiments, as it is observed in the paper alluded to, one of two inferences appears to be unavoidable; we must either admit the identity

of the nervous influence and galvanism, or that there is a power different from the nervous influence yet capable of performing its most complicated functions, a supposition, I think, much harder to be granted than the other.

Both this experiment, and the experiments on rabbits, relating to the same subject, were made in the presence of several medical Gentlemen, who expressed their entire satisfaction in the result.

I remain, Sir, your obedient humble servant,

C. P. Wilson Philip.

XIII. *New Spectacle Glasses.*

(To Dr. Thomson.)

Sir, *March* 6, 1816.

I have been informed that the French of late have been trying plano-cylindrical glasses with the plane surfaces in contact, and the axes at right angles. These have been used as lenses; and it is said that Dr. Wollaston is in possession of one of this construction. Any information which you can give your readers on this subject cannot fail to be interesting to them.

During the cold weather of January, 1814, a bubble of air (like that which you described a few months ago) was discharged from the spirits of wine of one of Six's thermometers. It measured 16·9°, and was reduced by the application of heat, although it was not then known that any such process had been used with success.

S.

The spectacle glasses alluded to by my correspondent consist of two segments of cylinders applied at right angles to each other. The object, I conceive, is to destroy the effect of spherical aberration. As far as I have been able to learn, this species of lens has not been found to answer well in this country, though I am not quite sure that they have undergone a fair trial.

XIV. *Uric Acid.*

Gay-Lussac mixed uric acid with 20 times its weight of peroxide of copper, put the mixture into a glass tube, and covered it with a quantity of copper filings. The copper filings, being heated to a dull red heat, heat was applied to the mixture. The gas which came over was composed of 0·69 carbonic acid and 0·31 azote. He conceives that the bulk of the carbonic acid would have been exactly double that of the azote had it not been for the formation of a little carbonate of ammonia. Hence uric acid contains two atoms of carbon and one atom of azote. This is the same proportion as exists in cyanogen. (Ann. de Chim. vol. xcvi. p. 53.) To what, then, are we to ascribe the difference between hydro-cyanic acid and uric acid? Does uric acid contain an atom of oxygen combined to cyanogen, while hydro-cyanic acid consists of the same base united to an atom of hydrogen?

XV. *Meteorological Table. Extracted from the Register kept at Kinfauns Castle, N. Britain. Supposed Lat.* 56° 23½′. *Above the Sea* 129 *feet.*

1815.	Morning, 8 o'clock. *Mean height of* Barom.	Morning, 8 o'clock. *Mean height of* Ther.	Even., 10 o'clock. *Mean height of* Barom.	Even., 10 o'clock. *Mean height of* Ther.	Mean by Six's Ther.	Mean of garden rain gage. In. 100	No. of days. Rain or Snow.	No. of days. Fair
Jan.	29·74	32·58	29·73	31·87	32·19	1·40	9	22
Feb.	29·47	40·00	29·49	40·07	40·71	2·20	15	13
March	29·36	39·96	29·38	39·61	41·06	2·40	18	13
April........	29·77	43·40	29·79	43·13	44·79	1·05	5	25
May	29·68	51·54	29·70	49·64	52·44	3·32	15	16
June	29·68	55·13	29·68	53·43	56·36	0·90	8	22
July	29·85	57·61	29·85	55·61	58·19	2·00	11	20
Aug.	29·62	56·83	29·61	55·51	57·90	1·60	8	23
Sept.	29·70	52·60	29·69	53·00	54·04	2·40	13	17
Oct.	29·62	47·41	29·59	47·90	48·80	3·82	15	16
Nov.	29·79	36·60	29·82	37·23	38·00	1·30	6	24
Dec.	29·55	33·00	29·56	33·00	33·10	1·81	9	22
Aver. of year.	29·6525	45·555	29·6575	45·00	46·465	24·20	132	233

ANNUAL RESULTS.

MORNING.

Barometer. *Observations.*	*Wind.*		Thermometer.	*Wind.*	
Highest, Nov. 26	S E	30·55	July 13................	S E	65°
Lowest, Mar. 13	W	28·55	Dec. 17................	W	17

EVENING.

Highest, Nov. 25	N W	30·55	July 12	S W	63°
Lowest, Dec. 28	N W	28·60	Dec. 19	N W	14

Weather.	*Days.*	*Wind.*	*Times.*
Fair........................	233	N and N E	9
Rain or Snow..........	132	E and S E	102
		S and S W	85
		W and N W	169
	365		365

Extreme Cold and Heat, by Six's Thermometer.

Coldest, 23d December, Wind S.......................... 12°
Hottest, 29th June, Wind S W 73
Mean of the year 1815 .... 46·465

Result of three Rain Gages. *In.* 100.

No. 1. On a conical detached hill above the level of the sea 600 feet.... 45·70
No. 2. Centre of garden, 20 feet .... 24·20
No. 3. Kinfauns Castle, 129 feet....................................... 18·00

Mean of the three gages....... .... 29·30

XVI. *Combination of Prussic Acid and Oil of Peaches.*

Brugnatelli, by distilling a great quantity of water on the leaves of the peach, obtained an oil, which was kept for eight years in a

well-stopped phial. At the end of that time a considerable number of crystals had separated from the oil. These crystals had the following properties:—

A strong smell of peach blossoms; a taste at first sweetish, then sharp and burning. The crystals were confused; but some of them were translucent plates. The specific gravity was 1·300. When thrown upon the surface of water, it moved about with great rapidity, like camphor in the same circumstances. Potash digested with this substance threw down Prussian blue from persulphate of iron. When exposed to the sun for some days, it separated from the oil with which it was contamina.ed. It then became white, lost its smell, was insoluble in water, but soluble in alcohol. The alcoholic solution, by spontaneous evaporation, gave very brilliant four-sided prisms, whose bases were squares. It was not acted on by nitric acid in the course of 24 hours. Brugnatelli considers this substance as a combination of prussic acid and oil of peaches. (Ann. de Chim. vol. xcvi. p. 96.)

Article XII.

New Patents.

Marquis de Chabannes, Russel-place, Fitzroy-square, London; for a method of conducting the air, and regulating the temperature, in houses and other buildings, and warming and cooling either air or liquids in a much more expeditious, and consequently less expensive, manner than hath hitherto been done within this kingdom, which is applicable to various useful purposes, and will be of great public advantage. Dec. 5, 1815.

Christoph Dihl, Frith-street, Soho, London; for certain improvements in the method or apparatus of distillation. Dec. 5, 1815.

James Lee, Old Ford, Middlesex; for certain improvements in the methods before invented by him of preparing hemp and flax; and by which also other vegetable substances may be rendered applicable to many of the purposes for which hemp and flax are used. Dec. 5, 1815.

Samuel Clegg, of the gas works, Peter-street, Westminster; for an improved gas apparatus. Dec. 9, 1815.

Davis Redmund, Johnson's-court, Fleet-street, London; for a machine for the manufacture of corks and bungs. Dec. 9, 1815.

Robert Kinder, Hill-street, Liverpool; for a method of propelling ships, boats, and other vessels. Dec. 9, 1815.

Robert Dickinson, Great Queen-street, Lincoln's Inn-fields, London; for an improvement in the hooping of barrels. Dec. 17, 1815.

Article XIII.

METEOROLOGICAL TABLE.

1816.	Wind.	Barometer. Max.	Min.	Med.	Thermometer. Max.	Min.	Med.	Hygr. at 9 a. m.	Rain.	
2d Mo.										
Feb. 20	N E	30·00	29·88	29·940	47	34	40·5	80		☾
21	W	30·07	30·00	30·035	46	36	41·0	75	4	
22	S W	30·19	30·07	30·130	49	34	41·5	80	—	
23	S	30·19	30·11	30·150	50	27	38·5	98		
24	S W	30·11	29·99	30·050	50	35	42·5	70		
25	S W	30·04	29·85	29·945	53	32	42·5	63	·12	
26										
27	W	29·75	29·45	29·600	51	29	40·0		8	
28	N W	29·88	29·75	29·815	40	22	31·0	53		●
29	N W	29·86	29·82	29·840	35	25	30·0	59		
3d Mo.										
March 1	S	29·82	29·43	29·625	41	28	34·5	75		
2		29·43	29·15	29·290	42	36	39·0	80	·25	
3	S W	29·25	29·06	29·155	45	29	37·0	76	·10	
4	S W	29·19	29·13	29·160	46	26	36·0	74	·21	
5	S	28·97	28·90	28·935	44	32	38·0	67	—	
6	S W	29·10	28·92	29·010	46	33	39·5	76	·34	
7	S	29·13	29·02	29·075	50	32	41·0	80	·27	☽
8	N E	29·32	28·99	29·155	43	33	38·0	75	·18	
9	N	29·67	29·32	29·495	38	30	34·0	62	—	
10	N W	29·87	29·86	29·865	41	26	33·5	77		
11	S W	29·65	29·62	29·635	50	41	45·5	90	·39	
12	S W	29·64	29·50	29·570	52	40	46·0	70	8	
13	W	29·87	29·82	29·845	52	38	45·0	83	—	○
14	S W	29·82	29·32	29·570	52	43	47·5	58	—	
15	S	29·76	29·32	29·540	52	26	39·0	73	·23	
16	W	29·74	29·67	29·705	49	30	39·5	60		
17	S W	29·74	29·49	29·615	47	34	40·5	78	—	
18	N W	29·56	29·43	29·495	52	35	43·5	51	·17	
19	N W	29·91	29·56	29·735	47	34	40·5		3	
		30·19	28·90	29·606	53	22	39·46	72	2·49	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Second Month.—20. Light clouds. 21. Several birds sing: *Cirrostratus* beneath large *Cirrus.* 22. Cloudy: drizzling: fair: windy. 23. White frost, which speedily went off: there appears to have been a dripping mist in the night: *Cirrocumulus:* fair. 24. *Cirrus: Cirrostratus:* cloudy: hollow wind. 25. A gale from S.W., with showers: changed to N.W. in the night. 27. A snow shower early, which was followed by sleet and rain: much wind in the night. 28. a. m. Light *Cirrostratus:* windy: *Cumulus* and *Cumulostratus* succeeded. 29. Slight hoar frost: fair, with light clouds: hygr. went to 42°.

Third Month.—2, 3. Rain at intervals. 4. a. m. *Cirri,* consisting of streamers rising from a horizontal base, with *Cirrostratus* below: heavy clouds: wind: p. m. Hail, sleet, rain: lastly, upon the wind getting somewhat northerly, a heavy short storm of snow. 5. a. m. Clear: the ground crusted with yesterday's snow. 6. a. m. Various modifications of cloud: p. m. heavy showers. 7. Cloudy: some rain, a. m.: *Nimbi.* 8. Rainy. 9. Snow storm: much evaporation: fair night. 10. Fair. 11. Stormy: very wet, p. m. 12. Temp. 50° at nine, a. m,: wind and rain. 13. Much wind: rain at intervals: *Nimbi.* 14. Wet morning: the wind S. E.: stormy day and night. 15. Much wind: rain, p. m.: calm at night. 16. Hoar frost: fair: calm: hygr. went to 45°; and although it was overcast through the day, with the usual indications of rain in the sky, yet none fell. 17. *Cirrus,* with other light clouds, a. m.: wet, p. m. 18. Wet morning: hollow southerly wind, which changed to N. W., with *Nimbi,* at night, and blew strong. 19. A raw blustering day, with much evaporation evident.

RESULTS.

Prevailing Winds Westerly.

Barometer:	Greatest height	30·19 inches
	Least	28·90
	Mean of the period	29·606
Thermometer:	Greatest height	53°
	Least	22
	Mean of the period	39·46

Mean of De Luc's hygrometer, 72°. Rain, 2·49 inches.

Character of the period: cloudy, wet, and windy: vegetation has made but little progress.

TOTTENHAM,
Third Month, 21, 1816.

L. HOWARD.

ANNALS

OF

PHILOSOPHY.

MAY, 1816.

Article I.

*Biographical Sketch of Alexander Wilson.**

ALEXANDER WILSON was born in the town of Paisley, in Scotland; † and received the elements of a classical education at a grammar school of his native place. About the age of ten he had the misfortune to lose his mother; and his father, who was closely engaged in the occupation of a distiller, feeling the necessity of an adjunct in the government of an infant family, again entered into the matrimonial state.

Young Wilson's father had designed him for a learned profession; but this intention, how agreeable soever to parental feelings, was not relished by the son, who had imbibed some prejudices, which were the cause of the project being abandoned.

The introduction of a step-mother into Mr. Wilson's family, as is too often the case, was productive of unhappiness. The subject of this memoir became the object of aversion, through some unknown cause, to his new guardian; who employed her influence to his disadvantage with such effect that the poor lad was compelled to forsake his paternal roof, and to seek an asylum under that of his brother-in-law, William Duncan, who resided at Queen's-ferry, on the Frith of Forth. Mr. Duncan was a weaver; and young Wilson, convinced by experience of the necessity of self-exertion, applied himself with diligence to acquire a knowledge of that trade, at which he continued for several years.

* From American Ornithology, vol. ix.—T.

† The year in which he was born could not be ascertained; but it is conjectured by his friends that he was about 45 at his decease.

At an early period of his life he evinced a strong desire for learning; and the perusal of old magazines and pamphlets, to which he had ready access, was an additional stimulus to further exertion. His mind, it is reasonable to conjecture, was not a little agitated at the solemn alternative of persecution, or of relinquishing for ever the fostering attentions of a parent, to whom he was most dutifully and affectionately attached; and he experienced consolation by devoting his leisure hours to reading and writing. Poetry attracted his regard; it was the vehicle of sentiments which were in unison with his sanguine feelings: he had early imbibed a love of virtue; and it now assumed a romantic cast, by assimilation with the high-wrought efforts of fancy, combined with the melody of song.

Caledonia is fruitful of versemen: every village has its poets; and so prevalent is the habit of jingling rhymes, that a scholar is considered as possessing no taste if he do not attune the Scottish lyre to those themes which the *amor patriæ*, the national pride of a Scotsman, has identified with his very existence.

Burns was now in the zenith of his glory. His verses were on the lips of every one; his praises were echoed from the cottage to the palace; and from the unexampled success of this humble son of genius, many aspired to the honours of the laurel, who otherwise would have confined their views of poetical renown to the limited circle of their family or acquaintance. Among this number may be reckoned our Wilson; who, finding from some short essays, that he possessed the talent of poetical expression, ventured to exhibit his attempts to his friends, whose approbation encouraged him to renewed perseverance, in the hope of emerging from that condition in society which his aspiring soul could not but disdain.

Mr. Duncan, with a view of bettering his estate, relinquished the occupation of weaving, and became a travelling merchant, or, in common language, a pedlar. In his expeditions young Wilson, now approaching to manhood, frequently accompanied him; and thus was a foundation laid of a love for travelling, which became a ruling passion with our author during the remainder of his existence.

Alexander was now left to shift for himself; and as he was completely initiated in the art of trading, he shouldered his pack, and cheerfully set out in quest of riches. In a mind of a romantic turn, Scotland affords situations abundantly calculated to arouse all those feelings which the sublime and beautiful in nature inspire. Wilson was a poetical enthusiast; and the bewitching charms of those mountains, valleys, and streams, long since immortalized in song, filled his soul with rapture, and enkindled all the efforts of his youthful muse. From a habit of contemplating the works of Nature, arose an indifference to the vulgar employment of trading, which became more disgusting at each interview with the muses; and nothing but the dread of poverty induced him to conform to the dull avocations of common life.

He occasionally contributed essays to various periodical publica-

tions, amongst which we may name the Bee, conducted at Edinburgh by Dr. Anderson. He likewise was in the habit of frequenting the Pantheon at the same place, wherein a society for debate held their meetings. In this assembly of wits he delivered several poetical discourses, which obtained him considerable applause.

In consequence of his literary attainments, and correct moral deportment, he was admitted to the society of several Gentlemen of talents and respectability, who descried in our youth the promise of future eminence. Flattered by attentions which are always grateful to the ingenuous mind, he was emboldened to the design of collecting and publishing his various poetical attempts: hoping thereby to realize funds sufficient to enable him to persevere in the walks of learning, which, to his glowing fancy, were profusely strewed with flowers.

The volume appeared under the title of Poems, Humorous, Satirical, and Serious, by Alexander Wilson. The writer of this sketch has it now before him; and finds in it the following remarks, in the hand-writing of the author himself: "I published these poems when only 22—an age more abundant in *sail* than *ballast*. Reader, let this soften the rigour of criticism a little." Dated Gray's Ferry, July 6, 1804. These poems were in truth the productions of a boy, who composed them under the most disadvantageous circumstances. They answered the purpose for which they were originally intended—to gratify the partiality of friendship, and soften moments of despondency. Their author, in his riper years, lamented his rashness in giving them to the world; and it is to be hoped that no one will be so officious as to draw them from that obscurity to which he, who gave them existence, sincerely rejoiced to see them condemned. These poems went through two small octavo editions, the last of which appeared in 1791. The author reaped no benefit from the publication.

About this period of his life the town of Paisley was agitated by a misunderstanding between the manufacturers and the weavers; and all the talents of both parties were exerted on the occasion. Young Wilson, attached to his side by the double tie of principle and interest, boldly espoused their cause, and was considered no mean champion in the controversy.

Amongst the manufacturers there was one of considerable wealth and influence, who had risen from a low origin by a concurrence of fortunate circumstances, and who had rendered himself greatly obnoxious by his avarice and knavery. Him our poetical weaver arraigned in a galling satire, written in the Scottish dialect, which of all languages is perhaps the most fertile of terms of sarcasm or abuse. The piece was published anonymously; and though Wilson was suspected to be the writer, yet no evidence could be adduced to establish the fact. But unfortunately as he was one night, at a late hour, returning from his printer, some spies, who had been watching his movements, seized upon him; and papers being found in his possession which indicated the author, he was prosecuted for a

libel, sentenced to a short imprisonment, and to burn, with his own hands, the piece at the public cross in the town of Paisley. The printer, it is said, was likewise fined for his share in the publication.

In the year 1792 Mr. Wilson wrote his characteristic tale entitled Watty and Meg. This little poem was published anonymously; and possessing considerable merit, was by many attributed to Burns. It has obtained more popularity in Scotland than any of the minor essays of our author; and has been ranked with the best productions of the Scottish muse.

He now began to be dissatisfied with his lot. He was poor, and saw no prospect of bettering his condition in his native country; and having heard flattering accounts of America, he conceived the design of forsaking the land of his forefathers, and settling in the United States. With this intention he arranged his affairs; set out for Belfast, in Ireland; engaged his passage in the ship Swift, of New York, Capt. Steel, bound to Philadelphia; and arrived at Newcastle, in the state of Delaware, on July 14, 1794.

We now behold Alexander Wilson in a strange land; without an acquaintance on whose counsels and hospitality he could rely in that state of uncertainty, to which, having no specific object in view, he was of course subjected; without a single letter of introduction; and with only a few shillings in his pocket. But every care was forgotten in his transport at finding himself in the land of freedom. He had often cast a wishful look towards the western hemisphere, and his warm fancy had suggested the idea that amongst that people only who maintained the doctrine of an equality of rights could political justice and happiness be found. He had become indignant at beholding the influence of the wealthy converted into the means of oppression; and had imputed the wrongs and sufferings of the poor, not to the condition of society, but to the nature and constitution of the government.* He was now free; and exulted in his release, as a bird rejoices which escapes from the confinement of the cage. Impatient to set his foot on the soil of the New World, he landed at Newcastle; and shouldering his fowling-piece, directed his route towards Philadelphia, distant about 33 miles. The writer of this biography has a distinct recollection of a conversation with Mr. Wilson on this part of his history, wherein he described his sensations on viewing the first bird that presented itself as he entered the forests of Delaware. It was a red-headed woodpecker, which he shot, and considered the most beautiful bird he had ever beheld.

On his arrival at Philadelphia he reflected on the most eligible mode of obtaining a livelihood, to which the state of his funds urged immediate attention. He made himself known to Mr. John Aitkin, a copper-plate printer, who on learning his situation gave him em-

* I do not know who the author of this life is, though, from various expressions, it is probable that he is a Scotchman.—T.

ployment at that business, at which he continued for a few weeks; and abandoned it for his trade of weaving, having made an engagement with Mr. Joshua Sullivan, who resided on the Pennypack creek, about 10 miles north of Philadelphia.

The confinement of the loom did not agree either with Mr. Wilson's habits or inclinations; and learning that there was considerable encouragement afforded to settlers in Virginia, he migrated thither, and took up his residence near Shepherd's Town, in that part of the state known by the name of New Virginia. Here he again found himself necessitated to engage in the same sedentary occupation; and soon becoming disgusted with the place, he returned to his friend, Mr. Sullivan, at Pennypack.

I find from one of his journals that in the year 1795 he travelled through the north part of the state of New Jersey, with an acquaintance, in the capacity of a trader, and met with tolerable success.

On his return from the above expedition he opened a school on the Bustle Town road, a short distance from the town of Frankfort, Pennsylvania. Being dissatisfied with this situation, he removed to Miles Town, and taught in the school-house of that village. In this last place he continued for several years; and being deficient in the various branches of learning necessary to qualify him for an instructor of youth, he applied himself to study with great diligence; and acquired all his knowledge of the mathematics, which was considerable, solely by his own exertions.

Whilst residing at Miles Town he made a journey on foot to the Gennesee country, for the purpose of visiting a small farm of which he was joint proprietor; and in the space of 28 days traversed an extent of nearly 800 miles.

He changed his residence next for one in the village of Bloomfield, New Jersey, where he again opened a school. But soon being advised of a more agreeable situation, he solicited and received an engagement from the trustees of Union School, in the township of Kingsess, a short distance from Gray's Ferry, on the river Schuylkill.

This removal constituted an important era in the life of Mr. Wilson. His school-house and residence being but a short distance from the botanical garden of Messrs. Bartram, situate on the western bank of the Schuylkill, a sequestered spot possessing attractions of no ordinary kind, an acquaintance was soon contracted with that venerable naturalist, Mr. William Bartram, which ripened into an uncommon friendship, and continued without the least abatement until severed by the hand of death. Here it was that Mr. Wilson found himself translated, if we may so speak, into a new existence. He had long been a lover of the works of Nature, and had derived more happiness from the contemplation of her simple beauties than from any other source of gratification. But he had hitherto been a mere novice; he was now about to receive instructions from one whom the experience of a long life, spent in

travel and rural retirement, had rendered qualified to teach. Mr. Bartram soon perceived the bent of his friend's mind, and its congeniality to his own; and took every pains to encourage him in a study which, while it expands the faculties, and purifies the heart, insensibly leads to the contemplation of the glorious Author of nature himself. From his youth Mr. Wilson had been observant of the manners of birds; and since his arrival in America had found them objects of uncommon interest; but he had not yet viewed them with the eye of a naturalist.

Mr. Bartram possessed some works on natural history, particularly those of Catesby and Edwards. Mr. Wilson perused them attentively; and found himself enabled, even with *his* slender stock of information, to detect errors and absurdities into which these authors had fallen, from a defective mode of studying nature: a mode which, while it led them to the repositories of dried skins and preparations, and to a reliance on hearsay evidence, subjected them to the imputation of ignorance, which their lives, devoted to the cultivation and promotion of science, certainly would not justify. Mr. Wilson's improvement was now rapid; and the judicious criticisms which he made on the above-mentioned authors, gratified his friend and instructor, who redoubled his encouraging assistance, in order to further one in a pursuit for which his genius, now beginning to develope itself, was evidently fitted.

In his new situation Mr. Wilson had many enjoyments; but he had likewise moments of despondency which solitude tended to confirm. He had addicted himself to the writing of verses and to music; and being of a musing turn of mind, had given way to those seductive feelings which the charming scenery of the country, in a susceptible heart, never fails to awaken. This was a fatal bias, which all his efforts could not counteract or remove. His friends perceived the danger of his state; and one in whose friendship he had placed strong reliance, and to whom he had freely unburthened himself, Mr. Lawson, the engraver, became alarmed for the soundness of his intellect.* There was one subject which contributed not a little to increase his mental gloom, and that was the consideration of the life of penury and dependance to which he seemed destined as the teacher of a country school. Mr. Lawson immediately recommended the renouncing of poetry and the flute, and the substituting of the amusement of drawing in their stead, as

* Since the above has been in type, the following incident has been communicated to us by Col. Carr, who had it from Mr. Wilson himself. During the time that the latter laboured under great depression of spirits, in order to sooth his mind he one day rambled with his gun. The piece by accident slipped from his hand, and in making an effort to regain it the lock was cocked. At that moment, had the gun gone off, it is more than probable that he would have lost his life, as the muzzle was opposite to his breast. When Mr. Wilson reflected on the danger which he had escaped, he shuddered at the idea of the imputation of suicide, which a fatal occurrence, to one in his frame of mind, would have occasioned. There is room to conjecture that many have accidentally met their end whose memories have been sullied by the alleged crime of self murder.

being most likely to restore the balance of his mind, and as an employment well adapted to one of his recluse habits and inclinations. To this end sketches of the human figure, and landscapes, were provided him; but his attempts were so unpromising that he threw them aside with disgust; and concluded that one at his period of life, being near 40, could never succeed in the art of delineation. His friend Mr. Bartram now advised a trial at birds; and being tolerably skilful himself, exhibited his port-folio, which was graced with many specimens from his own hands. The attempt was made, and succeeded beyond the expectation of Mr. Wilson, or that of his friends. There was a magic in the employment which aroused all the energies of his soul; he saw, as it were, the dayspring of a new creation; and from being the humble follower of his instructors, he was soon qualified to lead the way in the charming art of imitating the works of the Great Original.

If a momentary digression from our subject would be pardoned, the writer of this sketch would suggest the idea of erecting in that classical retreat, Bartram's Botanic Garden, a rural monument or altar, dedicated to the amiable Genius of Painting, as to her inspiration the world is indebted for the American Ornithology.

That Mr. Wilson likewise succeeded tolerably well in delineating flowers, appears from the following note to Mr. Bartram, dated Nov. 20, 1803:—

"I have attempted two of those prints which Miss Nancy * so obligingly, and with so much honour to her own taste, selected for me. I was quite delighted with the anemone, but fear I have made but bungling work of it. Such as they are I send them for your inspection and opinion; neither of them is quite finished. For your kind advice towards my improvement, I return my most grateful acknowledgments.

"The duties of my profession will not admit me to apply to this study with the assiduity and perseverance I could wish. Chief part of what I do is sketched by candle-light; and for this I am obliged to sacrifice the pleasures of social life, and the agreeable moments which I might enjoy in company with you and your amiable friend. I shall finish the other some time this week; and shall be happy if what I have done merit your approbation."

As Mr. Wilson advanced in drawing, he made corresponding progress in a knowledge of ornithology. He had attentively perused the works of the naturalists of Europe, who had written on the subject of the birds of America; and became so disgusted with their caricatured figures, fanciful theories, fables, and misrepresentations, that on turning, as he himself observes, from these barren and musty records to the magnificent repository of the woods and fields—the Grand Aviary of Nature, his delight bordered on adoration. It was not in the inventions of man, the reveries of the

* Mr. Bartram's niece, now the consort of Col. Carr, of the U. S. army.

closet philosopher, that the Divine Wisdom could be traced; but it was visible in the glorious volume of creation, on the pages of which are inscribed the Author's lessons of goodness and love, in the conformation, the habitudes, melody, and migrations, of the feathered tribes, that beautiful portion of the work of his hands.

To invite the attention of his fellow beings to a study attended with so much pleasure and improvement, was the natural wish of one who had been educated in the school of Wisdom. He humbly thought it would not be rendering an unacceptable service to the Great Master of Creation himself, to deduce from objects that every where present themselves in our rural walks, not only amusement and instruction, but the highest incitements to piety and virtue. Moreover, self-gratification, that source of so many of our most virtuous actions, had its share in urging him to communicate his observations to others. He examined the strength of his own mind and its resources; the undertaking seemed hazardous; he pondered it for a long while before he ventured to mention it to his friends. At length the subject was made known to Mr. Bartram, who freely expressed his confidence in the abilities and acquirements of Mr. Wilson, but from a knowledge of the situation and circumstances of the latter, hinted his fears that the difficulties which stood in the way of such an enterprise were almost too great to be overcome. Wilson was not easily intimidated; the very mention of difficulties suggested to his ardent mind the means of surmounting them, and the glory which would accrue from such an achievement. He had a ready answer to every objection of his cautious friend; and evinced such enthusiasm, that Mr. Bartram trembled lest his intemperate zeal should lead him into a situation, from the embarrassments of which he could not well be extricated.

The scheme was unfolded to Mr. Lawson, and met his unqualified approbation. But he observed that there were several considerations which should have their weight in determining in an affair of so much importance. These were frankly stated; and followed by advice, which did not quadrate with Wilson's temperament; who, vexed that his friend would not enter into his feelings, expressed his scorn of the maxims of prudence with which he was assailed, by styling them the offspring of a *cold, calculating, contemptible*, philosophy. Under date of March 12, 1804, he thus writes to the last-named Gentleman :—

" I dare say you begin to think me very ungenerous and unfriendly in not seeing you for so long a time. I will simply state the cause, and I know you will excuse me. Six days in one week I have no more time than just to swallow my meals, and return to my *Sanctum Sanctorum*. Five days of the following week are occupied in the same routine of pedagoguing matters; and the other two are sacrificed to that itch for drawing which I caught from your honourable self. I never was more wishful to spend an afternoon with you. In three weeks I shall have a few days vacancy, and

mean to be in town chief part of the time. I am most earnestly bent on pursuing my plan of making a collection of all the birds in this part of North America. Now I don't want you to throw cold water, as Shakspeare says, on this notion, quixotic as it may appear. I have been so long accustomed to the building of airy castles and brain windmills, that it has become one of my earthly comforts, a sort of a rough bone, that amuses me when sated with the dull drudgery of life."

In the month of October, 1804, Mr. Wilson, accompanied by two of his friends, set out on a pedestrian journey to visit the far-famed Cataract of Niagara, whereof he had heard much, but which he never before had an opportunity of beholding. The magnificent scenery of that beautiful river, as might be expected, filled the bosom of our poet with the most rapturous emotions. He gazed upon the cataract with an enthusiasm bordering upon distraction; and ever after declared that no language was sufficiently comprehensive to convey an adequate idea of that wonderful curiosity.

It is possible, by the force of description of a work of art, or common scene of nature, to raise the fancy to such a degree that the reality comes short of expectation. But of the Falls of Niagara it may with truth be observed that the utmost stretch of the imagination falls infinitely short of portraying the terrific sublimity of the mighty torrent.

On the return of Mr. Wilson he employed his leisure moments in writing a poetical narrative of the journey. This poem, which abounds with interesting description and pleasing imagery, is entitled The Forresters, and was gratuitously tendered to the proprietors of the Port Folio, and published in that excellent miscellany.

This expedition was undertaken rather too late in the season, and consequently our travellers were subjected to hardships of which they were not aware. Winter overtook them whilst in the Gennesee country, on their return by the way of Albany; and they were compelled to trudge the greater part of the route through snow mid-leg deep. Perhaps it may gratify the readers of the poem, which closes at the Falls of Niagara, to be informed, that of the colleagues of the author, one tarried amongst his friends on the Cayuga lake, and the other gave out, and took the benefit of a more agreeable mode of travelling. But the hardy Wilson's pride would not permit *him* to be overcome by fatigue or difficulties. He manfully kept the road, refusing to be relieved even of his gun and baggage; and arrived at his home the 7th of Dec., having been absent 59 days, and traversed in that time upwards of 1,200 miles. The last day he walked 47 miles.

The following letter to Mr. Bartram, illustrative of his views and feelings at this juncture, is interesting in a great degree:—

" *Gray's Ferry, Dec.* 15, 1804.

" Though now snugly at home, looking back in recollection on

the long, circuitous journey which I have at length finished, through trackless snows and uninhabited forests; over stupendous mountains, and down dangerous rivers; passing over, in a course of 1,300 miles, as great a variety of men and modes of living as the same extent of country can exhibit in any part of North America—though in this tour I have had every disadvantage of deep roads and rough weather, hurried marches, and many other inconveniences, to encounter—yet so far am I from being satisfied with what I have seen, or discouraged by the fatigues which every traveller must submit to, I feel more eager than ever to commence some more extensive expedition, where scenes and subjects entirely new and generally unknown might reward my curiosity, and where perhaps my humble acquisitions might add something to the stores of knowledge. For all the hazards and privations incident to such an undertaking, I feel confident in my own spirit and resolution. With no family to enchain my affections, no ties but those of friendship, and the most ardent love of my adopted country; with a constitution which hardens amidst fatigues; and a disposition sociable and open, which can find itself at home by an Indian fire in the depth of the woods as well as in the best apartment of the civilized. For these, and some other reasons that invite me away, I am determined to become a traveller. But I am miserably deficient in many acquirements absolutely necessary for such a character. Botany, mineralogy, and drawing, I most ardently wish to be instructed in, and with these I should fear nothing. Can I yet make any progress in botany sufficient to enable me to be useful? and what would be the most proper way to proceed? I have many leisure moments that should be devoted to this pursuit, provided I could have hopes of succeeding. Your opinion on this subject will confer an additional obligation on your affectionate friend."

It is worthy of remark that when men of uncommon talents project any great scheme they usually overlook those circumstances of minor importance, which ordinary minds would estimate as first deserving attention. Thus Wilson, with an intellect expanded by information, and still grasping at further improvement as a mean of distinction, would fain become a traveller, even at the very moment when the sum total of his funds amounted to 75 cents.! *

He now employed all his vacant hours in drawing and the study of ornithology, being resolutely bent on the accomplishing of his design, of which he became more enamoured the longer he reflected on it.

The spring of the year 1805 arrived, and gave to the enraptured view of our naturalist his interesting feathered acquaintance. He listened to their artless songs; he noted their habitudes; he sketched their portraits: and after having passed a few months varied with this charming occupation, he again writes to the respected inhabitant of the Botanic Garden:—

* This fact the editor had from one of Mr. Wilson's own letters.

"*Union School, July* 2, 1805.

"I dare say you will smile at my presumption, when I tell you that I have seriously begun to make a collection of drawings of the birds to be found in Pennsylvania, or that occasionally pass through it: 28 as a beginning I send for your opinion. They are, I hope, inferior to what I shall produce, though as close copies of the originals as I could make. One or two of these I cannot find either in your nomenclature, or among the seven volumes of Edwards. Any hint for promoting my plan, or enabling me to execute better, I will receive from you with much pleasure. I have resigned every other amusement, except reading and fiddling, for this design, which I shall not give up without making a fair trial.

"Criticise these, my dear friend, without fear of offending me —this will instruct, but not discourage me; for there is not among all our naturalists one who knows so well what they are, and how they ought to be represented. In the mean time accept of my best wishes for your happiness—wishes as sincere as ever one human being breathed for another. To your advice and encouraging encomiums I am indebted for these few specimens, and for all that will follow. *They may yet tell posterity that I was honoured with your friendship, and that to your inspiration they owe their existence.*"

The plates illustrative of the natural history of Edwards were etched by the author himself. Mr. Wilson had examined them very attentively, and felt assured that, with a little instruction in the art of etching, he could produce more perfect delineations; and would be enabled, by his superior knowledge of colouring, to finish the figures for his contemplated work in a style not inferior to his spirited and beautiful drawings from nature.

Mr. Lawson was of course consulted on this occasion, and cheerfully contributed his advice and assistance in the novel and difficult enterprise. Wilson procured the copper; and, his friend having laid the varnish and furnished the necessary tools, he eagerly commenced the important operation, on the successful termination of which his happiness seemed to depend.

Let the reader pause and reflect on the extravagance of that enthusiasm which could lead a person to imagine that, without any knowledge of an art derived from experience, he could at once produce that effect which is the result only of years of trial and diligence.

The next day after Mr. Wilson had parted from his preceptor, the latter, to use his own words, was surprised to behold him *bouncing* into his room, crying out, "*I have finished my plate! let us bite it in with the aquafortis at once, for I must have a proof before I leave town!*" * Lawson burst into laughter at the ludicrous

* For the information of those of our readers who are unacquainted with the process of etching, we subjoin the following explanatory note:—

appearance of his friend, animated with impetuous zeal; and to humour him granted his request. The proof was taken, but fell far short of Mr. Wilson's expectations, or of his ideas of correctness. However, he lost no time in conferring with Mr. Bartram, to whom he wrote as follows :—

"*Nov.* 29, 1805.

"I have been amusing myself this some time in attempting to *etch*; and now send you a proof sheet of my first performance in this way. Be so good as communicate to me your own corrections, and those of your young friend and pupil. I will receive them as a very kind and particular favour. The drawings which I also send, that you may compare them together, were done from birds in full plumage, and in the best order. My next attempt in etching will perhaps be better, every thing being new to me in this. I will send you the first impression I receive after I finish the plate."

In a short time another plate was prepared, and completed with the despatch of the former. In fulfilment of his promise to his friend, he transmits a proof, accompanied with the following note:

"Mr. Wilson's affectionate compliments to Mr. Bartram, and sends for his amusement and correction another proof of his birds of the United States. The colouring being chiefly done last night, must soften criticism a little. Will be thankful for my friend's advice and correction.

"Mr. Wilson wishes his beloved friend a happy new year, and every blessing.

"*Saturday, Jan.* 4, 1806."

These essays in etching, though honourable to Mr. Wilson's ingenuity and perseverance, yet by no means afforded satisfaction. He became now convinced that the *point* alone was not sufficient to produce the intended effect, and that nothing short of the accuracy of the *graver* would in anywise correspond to his ideas of excellence. But in the delightful art of engraving he had never been instructed; and he could not command means sufficient to cover the expense of the plates even of a single volume, on the magnificent plan which his comprehensive mind had delineated. A proposition was now made to Mr. Lawson to engage in the work on a joint concern. But there were several reasons which this Gentleman adduced, sufficiently weighty, in his opinion, to warrant his non-acceptance of the offer. Mr. Wilson finding his schemes thus baffled, declared, with solemn emphasis, his resolution of proceed-

On the polished copper-plate a coat of varnish, of a particular composition, is thinly spread. The design is then traced, and cut through to the copper with an instrument termed a point. A bank of wax is now raised around the plate, and aquafortis poured into the enclosure, which eats into the copper only where the point has passed. The length of time requisite for the successful action of the aquafortis must be determined by the judgment of the operator.

ing alone in the publication, if it even cost him his life. "*I shall at least leave,*" continued he, "*a small beacon to point out where I perished.*"

About the commencement of this year information was disseminated, through the medium of the public prints, that the President of the United States had it in contemplation to despatch parties of ingenious men for the purpose of exploring the waters of Louisiana. Mr. Wilson, aroused at the intelligence, now conceived that a favourable opportunity was afforded him of gratifying a desire, which he had long indulged, of visiting those regions, which he was well convinced were rich in the various objects of science; and particularly where subjects, new and interesting, might be collected for his embrio work on the ornithology of our country. He expressed his wishes to Mr. Bartram, who approved of them; and the latter cheerfully wrote a letter to his friend and correspondent, Mr. Jefferson, wherein Mr. Wilson's character and acquirements were distinctly stated, recommending him as one highly qualified to be employed in that important national enterprise. This introductory, couched in the most gentlemanly terms, covered an application from Mr. Wilson himself, which, as faithful biographers of our deceased friend, we here think proper to insert entire:—

"*To his Excellency Thomas Jefferson, President of the United States.*

"Sir,

"Having been engaged these several years in collecting materials and furnishing drawings from nature, with the design of publishing a new orthinology of the United States of America, so deficient in the works of Catesby, Edwards, and other Europeans, I have traversed the greater part of our northern and eastern districts; and have collected many birds undescribed by these naturalists. Upwards of 100 drawings are completed, and two plates in folio already engraved. But as many beautiful tribes frequent the Ohio, and the extensive country through which it passes, that probably never visit the Atlantic states; and as faithful representations of these can only be taken from living nature, or from birds newly killed, I had planned an expedition down that river, from Pittsburg to the Mississippi, thence to New Orleans, and to continue my researches by land in return to Philadelphia. I had engaged as a companion and assistant Mr. William Bartram, of this place, whose knowledge of botany, as well as zoology, would have enabled me to make the best of the voyage, and to collect many new specimens in both those departments. Sketches of these were to have been taken on the spot, and the subjects put in a state of preservation to finish our drawings from, as time would permit. We intended to set out from Pittsburg about the beginning of May, and expected to reach New Orleans in September.

"But my venerable friend Mr. Bartram taking into more serious

consideration his advanced age, being near 70, and the weakness of his eye-sight; and apprehensive of his inability to encounter the fatigues and deprivations unavoidable in so extensive a tour; having, to my extreme regret, and the real loss of science, been induced to decline the journey; I had reluctantly abandoned the enterprise, and all hopes of accomplishing my purpose; till hearing that your Excellency had it in contemplation to send travellers this ensuing summer up the Red River, the Arkansaw, and other tributary streams of the Mississippi; and believing that my services might be of advantage to some of these parties, in promoting your Excellency's design; while the best opportunities would be afforded me of procuring subjects for the work which I have so much at heart. Under these impressions I beg leave to offer myself for any of these expeditions, and can be ready at a short notice to attend your Excellency's orders.

"Accustomed to the hardships of travelling, without a family, and an enthusiast in the pursuit of natural history, I will devote my whole powers to merit your Excellency's approbation, and ardently wish for an opportunity of testifying the sincerity of my professions, and the deep veneration with which I have the honour to be,

"Sir, your obedient servant,

"*Kingsess, Feb.* 6, 1806. "ALEX. WILSON." *

Mr. Jefferson had in his port-folio decisive proofs of Mr. Wilson's talents as an ornithologist, the latter having some time before transmitted to his Excellency some splendid drawings of nondescript birds, accompanied with scientific descriptions. Yet with these evidences before him, backed by the recommendation of a discerning and experienced naturalist, so little did Mr. Jefferson regard the pretensions of genius, and the interests of science; so unmindful was he of the duties of his exalted station, or the common civilities which obtain amongst people of breeding and refinement; that so far from accepting the services of our accomplished ornithologist, he did not even deign to *reply* to his respectful overture; and Wilson, mortified at the cold, contemptuous neglect, locked up his feelings in his breast, not even permitting a sigh to reach the ear of his most intimate friends. This treatment he did not expect from one whom his ardent fancy had invested with every excellence, who had been the object of his encomiums, and the theme of his songs:

"Omne ignotum pro magnifico."

* Mr. Wilson was particularly anxious to accompany Pike, who commenced his journey from the cantonment on the Missouri, for the sources of the Arkansaw, &c. on the 15th July, 1806.

(*To be continued.*)

Article II.

Some Observations on the Relations between the Specific Gravity of Gaseous Bodies and the Weights of their Atoms. By Thomas Thomson, M.D. F.R.S.

From the numerous papers upon the atomic theory which I have inserted in the successive volumes of the *Annals of Philosophy*, and from the paper by Berzelius on the theory of volumes, published in the same Journal, I take it for granted that my readers are acquainted with the outlines of both of these theories. Dr. Prout, in a very valuable paper published in the sixth volume of the *Annals*, has endeavoured to show that the specific gravity of any body may be obtained by multiplying the weight of its atom by half the specific gravity of oxygen gas. This is the same thing as to say that the weight of an atom of every body is always double its specific gravity in the state of gas. As the theory of volumes is exceedingly convenient in chemical experiments, I conceive that it will be interesting to practical chemists to see in one view the very simple relations which exist between the specific gravities of gaseous bodies and the weights of their atoms.

If we examine all the substances which can be exhibited in a gaseous state, and with the weight of the atoms of which we are acquainted with tolerable accuracy, we shall find that they may be divided into three sets. In the first set the specific gravity of the body, and the weight of its atom, are represented by the same number. In the second set, the weight of an atom is double that of the specific gravity, or of the weight of a volume. And in the third set the weight of an atom is equal to four times the specific gravity, or to four times the weight of a volume of the respective bodies.

In order to make this comparison, it is necessary to reduce the specific gravities to the same standard as the weights of the atoms. As we have chosen 1 to represent the weight of an atom of oxygen, we must employ the same number to represent the specific gravity of that body, and reduce the specific gravity of all the other gaseous bodies in that proportion.

The following table exhibits what I consider as the specific gravity of the different gaseous bodies, according to the present state of our knowledge:—

	Specific Gravity.	Weight of 100 cubic inches in grains.
Air	1·000	30·5
Hydriodic acid gas	4·429	135·084
Phosgene gas	3·439	104·891
Chlorine	2·500	76·250
Euchlorine	2·440	74·420
Sulphurous acid	2·222	67·771

	Specific Gravity.	Weight of 100 cubic inches in grains.
Cyanogen	1·801	54·933
Nitrous oxide	1·5278	46·598
Carbonic acid	1·527	46·373
Muriatic acid	1·284	39·162
Sulphureted hydrogen	1·180	35·890
Sulphur	1·111	33·888
Oxygen	1·111	33·888
Nitrous gas	1·0416	31·769
Olefiant gas	0·974	29·72
Azote	0·9722	29·652
Carbonic oxide	0·972	29·652
Hydro-cyanic acid vapour	0·937	28·58
Steam	0·625	18·062
Ammonia	0·590	18·000
Carbureted hydrogen	0·555	16·99
Carbon	0·416	12·688
Hydrogen	0·0694	2·117

I shall now arrange these different bodies in the three classes to which they respectively belong, reducing the specific gravities in the first column of the preceding table to the numbers which will represent them when we suppose the specific gravity of a volume of oxygen gas to be 1.

Set First.—Bodies having the weight of their atoms equal to the specific gravity of their volumes:—

	Sp. gr. oxygen being 1.	Weight of an atom.
Oxygen	1·000	1·000
Olefiant gas	0·876	0·875

Set Second.—Bodies having the weight of their atoms twice the specific gravity of their volumes:—

	Sp. gr. oxygen being 1.	Weight of an atom.
Phosgene gas	3·095	6·190
Chlorine	2·250	4·500
Sulphurous acid	2·000	4·000
Cyanogen	1·621	3·242
Nitrous oxide	1·375	2·750
Carbonic acid	1·374	2·750
Sulphureted hydrogen	1·062	2·124
Sulphur	1·000	2·000
Azote	0·875	1·750
Carbonic oxide	0·875	1·750
Steam	0·5625	1·125
Carbureted hydrogen	0·4995	0·999
Carbon	0·375	0·750
Hydrogen	0·0625	0·125

Set Third.—Bodies having the weight of their atoms four times the specific gravity of their volumes:—

	Sp. gr. oxygen being 1.	Weight of an atom.
Hydriodic acid	3·986	15·944
Muriatic acid	1·1557	4·623
Nitrous gas	0·9375	3·750
Hydro-cyanic acid	0·8433	3·3732
Ammonia	0·53125	2·125

From these tables it is obvious that there exists a very simple relation between the specific gravity of gaseous bodies and the weight of their atoms. The weight of the atom is either equal to the specific gravity of the gas, or twice that weight, or four times that weight. It seems to follow from this that the ultimate atoms of bodies differ in their weight, and that the ratio of their weights may be determined by the specific gravity.

The specific gravity of olefiant gas is twice as great as might have been expected. Hence it is obvious that the volume of carbon and the volume of hydrogen, of which it is composed, must be reduced to half a volume. This is not the case with any of the other binary compounds. This is the reason why the weight of its atom appears equal to the specific gravity of the gas.

The atom of all the simple substances (oxygen excepted), namely, chlorine, sulphur, azote, carbon, and hydrogen, is double the specific gravity. This is the law which Dr. Prout pointed out as belonging to all bodies. It will probably be found to apply to all simple bodies except oxygen. The weight of an atom of carbon and of sulphur was obtained by subtracting the specific gravity of oxygen from that of carbonic acid, and the specific gravity of hydrogen from that of sulphureted hydrogen; because it is known that oxygen may be changed into carbonic acid, and hydrogen into sulphureted hydrogen, without undergoing any alteration in their bulk.

The composition of the compound bodies belonging to the second class is as follows:—

Sulphurous acid, composed of	1 vol.	sulphur	+	1 vol.	oxygen.
Carbonic acid	1	carbon	+	1	oxygen.
Nitrous oxide	1	azote	+	$\frac{1}{2}$	oxygen.
Carbonic oxide	1	carbon	+	$\frac{1}{2}$	oxygen.
Steam	1	hydrog.	+	$\frac{1}{2}$	oxygen.
Sulphureted hydrogen	1	sulphur	+	1	hydrog.
Carbureted hydrogen	1	carbon	+	$\frac{1}{2}$	hydrog.
Cyanogen	1	carbon	+	$\frac{1}{2}$	azote.

These fractional numbers disappear when we consider these compounds as composed of atoms, in consequence of the weight of the atom being double that of the volume.

The first four compounds of the third set consist of gaseous compounds which unite without undergoing any condensation. Of

course their specific gravity is the mean of that of the constituents. Hydriodic acid, muriatic acid, and hydro-cyanic acid, are composed each of one volume of hydrogen united to one volume of iodine, chlorine, and cyanogen, respectively; so that the specific gravity of each is a mean of the two substances of which it is composed. Nitrous gas is composed of two atoms of oxygen and one of azote. Hence its specific gravity is a mean of that of twice the specific gravity of oxygen + the specific gravity of azote. Ammonia is composed of three volumes of hydrogen and one volume of azote condensed into two volumes; so that its specific gravity = $\frac{3 \times \cdot 0625 + 0 \cdot 875}{2} = 0 \cdot 53125$.

I have omitted euchlorine in the preceding enumeration, because it presents an anomaly. Its specific gravity is 2·44, or (supposing the specific gravity of oxygen to be 1) 2·196. Now the weight of its atom is obtained by multiplying this specific gravity by $2\frac{1}{2}$; for $2 \cdot 196 \times 2 \cdot 5 = 5 \cdot 480$; and the weight of an atom of it, supposing it composed of one atom chlorine (4·498) and one atom oxygen (1), is 5·498. If this fractional factor continue, after the nature of chlorine has been determined with more rigour than could be expected from the original experiments of Davy, it will show that the ratio between the specific gravity of gaseous bodies and the weight of their atoms, is not always quite so simple as it seems to be from the preceding tables; but the determination of this point must be left to future experimenters.

Article III.

Demonstration of the Binomial Theorem for Fractional and Negative Exponents. By Dr. ***.

The binomial theorem requires, according to the nature of the exponent, different demonstrations. In the case of the exponent being an entire number, we have an expansion consisting of a finite number of terms; whereas in the other cases, of its being either a negative quantity, or a fraction, the expansion consists of an infinite number of terms. The first case may be satisfactorily proved by the theory of combinations, as James Bernouilli has done; or by showing the general truth of the law by successive multiplications. But for the other cases these methods entirely fail; and the demonstrations that are usually given of the law of expansion in these cases are far from being complete. Some have derived the *general* demonstration from the theory of fluxions. Without examining whether the theory of fluxions can be proved without the assistance of this theorem, we shall remark that so elementary and important a problem ought, if possible, to be proved before the theory of

fluxions is established; and however ingenious these demonstrations may be, they cannot be admitted as the proper demonstrations by which the truth is first to be established. To the common demonstrations given in elementary books, we object that they are not *general.* The law of the coefficients is shown for the first coefficients, and this induction is generalized without any solid demonstration. (Nov. Comm. Petr. vol. xix. p. 103; Phil. Trans. 1806, p. 318.) A demonstration for those cases, which is at once satisfactory and elementary, seems, therefore, not to be generally known, and may be very desirable. We intend to submit to the judgment of the reader one which appears to us to have these characteristics, and is, as far as we know, new.* We shall suppose that it has been proved that n, being an entire number,

$(a + b)^n = a^n + n\, a^{n-1}\, b + \frac{n \,.\, n-1}{1 \,.\, 2}\, a^{n-2}\, b^2$ &c. .... $+$ $\frac{n \,.\, n-1 \ldots\ldots n-r+1}{1 \,.\, 2 \quad . \quad 3}\, a^{n-r}\, b^r$, &c.; and shall prove that, n being any number, the same expansion is true.

For the sake of abridgment, we shall denote the binomial coefficients $\frac{n \,.\, n-1 \,.\, n-2 \ldots . n-r+1}{1 \,.\, 2 \,.\, 3 \quad . \quad r}$ thus ${}^r\mathrm{I}^n$, where the number on the right denotes the number of factors both in the numerator and denominator, and the number on the left expresses the first factor, n, or the exponent of the binomial quantity. We have, therefore, generally, the following relation between two such coefficients:—

(α) $\frac{{}^r\mathrm{I}^n\; n-r}{r+1} = {}^{r+1}\mathrm{I}^n$ or ${}^r\mathrm{I}^n\; n - r = r + 1\; {}^{r+1}\mathrm{I}^n$ (β) whatever number n may be, and the theorem for n an entire number, is thus expressed:—

$(a + b)^n = a^n + {}^1\mathrm{I}^n\, a^{n-1}\, b + {}^2\mathrm{I}^n\, a^{n-2}\, b^2 +$ and ${}^r\mathrm{I}^n\, a^{n-r}\, b^3$ &c. $+ b^n$.

We shall now demonstrate that, whatever p or q may be, we have always

${}^r\mathrm{I}^{p+q} = {}^r\mathrm{I}^p + {}^{r-1}\mathrm{I}^p \,.\, {}^1\mathrm{I}^q + {}^{r-2}\mathrm{I}^p \,.\, {}^2\mathrm{I}^q + {}^{r-3}\mathrm{I}^p \,.\, {}^3\mathrm{I}^q$ &c. ...... ${}^{r-5}\mathrm{I}^p\; {}^5\mathrm{I}^q +$ &c. .. ${}^r\mathrm{I}^q$.

For $r = 1$, it will be easily seen that ${}^1\mathrm{I}^{p+q} = {}^1\mathrm{I}^p + {}^1\mathrm{I}^q$.

* We say that the demonstration is new, because we believe the demonstration in that general form in which we have given it to be new. Euler's demonstration in Novi Comment. Acad. Petrop. 19, is indeed much the same, as far as it goes; but Euler shows only the form of the first two coefficients, and says, Quemadmodum hic duos primos coefficientes per literas m et n determinare licebat, ita manifestum est, si superior multiplicatio ulterius continuaretur inde etiam sequentes coefficientes C, D, E, per easdem literas m et n definiri posse *quamvis calculus mox ita foret molestus ut maximum laborem requireret.* It is evident that the agreeing of the first two coefficients with the same coefficients for entire numbers, which is Euler's demonstration, cannot be satisfactory and strict. The same objection applies with equal force to Dr. Robertson's demonstration, which seems nearly to agree with that of Euler in the paper above referred to.

For $r = 2$ we have ${}^2I^{p+q} = (\alpha)\ {}^1I^{p+q}\frac{p+q-1}{2} = ({}^1I^p + {}^1I^q)$ $\frac{p+q-1}{2} = {}^1I^p\left(\frac{p-1}{2}\right) + \frac{{}^1I^p\,{}^1I^q}{2} + {}^qI\,\frac{q-1}{2} + \frac{{}^1I^q\,.\,{}^1I^p}{2} = {}^2I^p +$ ${}^1I^p\,.\,{}^1I^q + {}^2I^q.$

For $r = 3$ we have ${}^3I^{p+q} = {}^2I^{p+q}\,\frac{p+q-2}{3}\,(\alpha) = ({}^2I^p + {}^1I^p\,.$ ${}^1I^q + {}^2I^q)\,\frac{p+q-2}{3} = {}^2I^p\,\frac{p-2}{3} + {}^2I^p\,\frac{q}{3} + \frac{{}^1I^p\,{}^1I^q\,p-1}{3} + {}^1I^p\,.$ ${}^1I^q\,\frac{q-1}{3} + {}^2I^q\,\frac{q-2}{3} + {}^2I^q\,\frac{p}{3} = {}^3I^p + \frac{{}^2I^p\,{}^1I^q}{3} +^2 \frac{{}^2I^p\,.\,{}^1I^q}{3} +^2$ $\frac{{}^1I^p\,{}^2I^q}{3} + {}^3I^q + \frac{{}^2I^q\,{}^1I^p}{3} = {}^3I^p + {}^2I^p\,{}^1I^q + {}^1I^p\,{}^2I^q + {}^3I^q$, as it ought to be.

In the same manner we may prove that

${}^4I^{p+q} = {}^4I^p + {}^3I^p\,{}^1I^q + {}^2I^p\,{}^2I^q + {}^1I^p\,{}^3I^q + {}^4I^q.$

But in order to generalize this induction, and to show that this must always be so, we shall suppose that, proceeding in this manner, we had convinced ourselves of its truth up to the value s of r, so that we had proved that

${}^sI^{p+q} = {}^sI^p + {}^{s-1}I^p\,{}^1I^q + {}^{s-2}I^p\,{}^2I^q + {}^{s-3}I^p\,{}^3I^q$, &c.

And we shall prove that it will also be true for $r = s + 1$; for ${}^{s+1}I^{p+q} = {}^sI^{p+q}\,\frac{p+q-s}{s+1} =$ (substituting the value of ${}^sI^{p+q}$) $({}^sI^p + {}^{s-1}I^p\,{}^1I^q + {}^{s-2}I^p\,{}^2I \dots {}^{s-t}I^p\,{}^tI^q$, &c. $+ {}^sI^q)\,\frac{p+q-s}{r+1}$.

By multiplying every term by $\frac{p+q-s}{s+1}$, and writing the latter successively $\frac{(p-s+1)+(q-1)}{s+1}$, $\frac{(p-s+2)+(q-2)}{s+1}$, $\frac{(p-s+3)+(q-3)}{s+1}$, &c. we obtain

$${}^sI^p\,\frac{p-s}{s+1} + \frac{{}^sI^p\,{}^1I^q}{s+1} + \frac{{}^{s-1}I^p\,{}^1I^q\,q-1}{s+1}$$
$$+ \frac{{}^{s-1}I^p\,p-s+1}{s+1}\,{}^1I^q + \frac{{}^{s-2}I^p\,.\,{}^2I^q\,p-s+2}{s+1}.$$

The first term is ${}^{s+1}I^p\ (\alpha)$. We have also

$$\frac{{}_sI_p\,{}^1I^q}{s+1} + \left(\frac{{}^{s-1}I^p\,p-s+1}{s+1}\,.\,{}^1I^q\right)\,s\,\frac{{}^sI^p\,{}^1I^q}{s+1}\,(\beta) = {}^sI^p\,{}^1I^q$$
$$\frac{{}_s{}^{-1}I^p\,{}^1I^q\,q-1}{s+1} + \frac{{}_s{}^{-2}I^p\,{}^2I^q\,p-s+2}{s+1} = (\beta)$$
$$2\,\frac{{}^{s-1}I^p\,{}^2I^q + s - 1\,{}^{s-1}I^p\,{}^2I^q}{s+1} = {}^{s-1}I^p\,{}^2I^q.$$

And thus two terms produced by two successive terms will always give together the same terms in the product. Thus in the products

$({}^{s-t}\mathrm{I}^{p}\ {}^{t}\mathrm{I}^{q})\ \left(\frac{p-s+t+q-t}{s+1}\right) = (\beta)\ s-t+1\ \frac{{}^{s-t+1}\mathrm{I}^{p}\,{}^{t}\mathrm{I}^{q}}{s+1}$

$+\ t+1\ \frac{{}^{t+1}\mathrm{I}^{q}\ \ {}^{s-t}\mathrm{I}^{q}}{s+1}$ and ${}^{s-t-1}\mathrm{I}^{p}\ {}^{t+1}\mathrm{I}^{q}$

$\frac{p-s+t+1\ q-t-1}{s+1} = s-t\,\frac{{}^{t+1}\mathrm{I}^{q}\ \ {}^{r-t}\mathrm{I}^{p}}{s+1} + t+2\,\frac{{}^{s-t-1}\mathrm{I}^{p}\ \ {}^{t+2}\mathrm{I}^{q}}{s+1}.$

two parts give together ${}^{t+1}\mathrm{I}^{q}\ .\ {}^{s-t}\mathrm{I}^{p}$.

And it follows that the whole product will be

${}^{s+1}\mathrm{I}^{p} + {}^{s}\mathrm{I}^{p}\,.\,{}^{1}\mathrm{I}^{q} + {}^{s-1}\mathrm{I}^{p}\ {}^{2}\mathrm{I}^{q} + {}^{s-2}\mathrm{I}^{p}\ {}^{3}\mathrm{I}^{q}$, &c. ${}^{s+1}\mathrm{I}^{q}$,

which is of the same form. If, therefore, the proportion be true for $r = s$, it will also be true for $r = s + 1$. But as we proved it to be true for $s = 1$, $s = 2$, $s = 3$, it follows hence that it will be true for all succeeding values of r, or that it will be generally true.

Let us next assume two quantities of this form :—

$a^{p} + {}^{1}\mathrm{I}^{p}\,a^{p-1}\,b + {}^{2}\mathrm{I}^{p}\,a^{p-2}\,b^{2} + {}^{3}\mathrm{I}^{p}\,a^{p-3}\,b^{3}$, &c. and

$a^{q} + {}^{1}\mathrm{I}^{q}\,a^{p-1}\,b + {}^{2}\mathrm{I}^{q}\,a^{p-2}\,b^{2} + {}^{3}\mathrm{I}^{q}\,a^{p-3}\,b^{3}$, &c.

Where p and q may be any positive numbers, and multiply them together, the product will be as follows :—

$$a^{p+q} + \left.\begin{matrix}{}^{1}\mathrm{I}^{p}\\ +\ {}^{1}\mathrm{I}^{q}\end{matrix}\right\}\ a^{p+q-1}\,b\ \left.\begin{matrix}{}^{2}\mathrm{I}^{p}\\ +\quad {}^{2}\mathrm{I}^{p}\\ {}^{1}\mathrm{I}^{p}\,.\,{}^{1}\mathrm{I}^{q}\end{matrix}\right\}\ a^{p+q-2}\,b^{2}\ \left.\begin{matrix}{}^{3}\mathrm{I}^{p}\\ +\quad {}^{2}\mathrm{I}^{p}\ {}^{1}\mathrm{I}^{q}\\ {}^{1}\mathrm{I}^{p}\ {}^{2}\mathrm{I}^{q}\\ {}^{3}\mathrm{I}^{q}\end{matrix}\right\}\ a^{p+q-3}\,b^{3}\ \text{\&c.} +$$

$$\left.\begin{matrix}{}^{r}\mathrm{I}^{p}\\ {}^{r-1}\mathrm{I}^{p}\ {}^{1}\mathrm{I}^{q}\\ {}^{r-2}\mathrm{I}^{p}\ {}^{2}\mathrm{I}^{q}\\ {}^{r-3}\mathrm{I}^{p}\ {}^{3}\mathrm{I}^{q}\\ \text{\&c.}\\ {}^{r}\mathrm{I}^{q}\end{matrix}\right\}\ a^{p+q-r}\,b^{r}.$$

It will now be readily seen that the coefficient of $a^{p+q-r}\,b^{r}\ n = {}^{r}\mathrm{I}^{p+q}$, and that the whole product will be

$a^{p+q} + {}^{1}\mathrm{I}^{p+q}\ a^{p+q-1}\,b + {}^{2}\mathrm{I}^{p+q}\ a^{p+q-2}\,b^{2}$, &c. $+\ {}^{r}\mathrm{I}^{p+q}$ $a^{p+q-r}\,b^{5}$, &c. which, being still of the same form as the factors which produced it, gives, if multiplied by another quantity of the same form,

$a^{r} + {}^{1}\mathrm{I}^{r}\ a^{r-1}\ b + {}^{2}\mathrm{I}^{r}\ a^{r-2}\ b^{2}$, &c. the product

$a^{p+q+r} + {}^{1}\mathrm{I}^{p+q+r}\ a^{p+q+r-1}\ b + {}^{2}\mathrm{I}^{p+q+r}\ a^{p+q+r-2}\ b^{2}$, &c.

which is still of the same form. This being multiplied again by a quantity of the same form, would produce a quantity of a similar form. Let us now suppose that all the p, q, r, &c. are equal, let that number $= n$, and the sum $n\,p = m$ an entire number, so that $p = \frac{m}{n}$, and all those quantities which are multiplied will be equal, and we shall have for their product

$[a^{p} + {}^{1}\mathrm{I}^{p}\ a^{p-1}\ b + {}^{2}\mathrm{I}^{p-2}\ b^{2}$, &c.$]^{n} = a^{np} + {}^{1}\mathrm{I}^{np}\ a^{np-1}\,b +$

$^2I\ a^{np-2}\ b^2$, &c. and consequently putting for $n.p$, m; and for p, $\frac{m}{n}$.

$$a^{\frac{m}{n}} + {}^1I^{\frac{m}{n}}\ a^{\frac{m}{n}-1}\ b + {}^2I^{\frac{m}{n}}\ a^{\frac{m}{n}-2}\ b^2, \&c. = \sqrt[n]{}\ (a^m + {}^1I^m\ a^{m-1}\ b + {}^2I\ a^{m-2}\ b^2 + \&c.$$

but by the binomial theorem for entire numbers, the part on the left under the radical sign is $= (a + b)^m$, and therefore

$$a^{\frac{m}{n}} + {}^1I^{\frac{m}{n}}\ a^{\frac{m}{n}-1}\ b + {}^2I^{\frac{m}{n}}\ a^{\frac{m}{n}-2}\ b^2, \&c. = \sqrt[n]{(a + b)^m} = (a + b)^{\frac{m}{n}}$$

which is the binomial formula for fractional exponents. As

$$(a^{p+q} + {}^1I^{p+q}\ a^{p+q-1}\ b + {}^2I^{p+q}\ a^{p+q-2}\ b^2)\ (a^r + {}^1I^r\ a^{r-1}\ b + {}^2I^r\ a^{r-2}\ b^2)$$
$$= (a^{p+q+r} + {}^1I^{p+q+r}\ a^{p+q+r-1}\ b + {}^2I^{p+q+r}\ a^{p+q+r-2}\ b^2, \&c.$$

we have, putting $p + q + r = w$, and $p + q = w - r$,

$$a^{w-r} + {}^1I^{w-r}\ a^{w-r-1}\ b + {}^2I^{w-r}\ a^{w-r-2}\ b^2, \&c. = \frac{a^w + {}^1I^w\ a^{w-1}\ b + {}^2I^w\ a^{w-2}\ b^2 + \&c.}{a^r + {}^1I^r\ a^{r-1}\ b + {}^2I^r\ a^{r-2}\ b^2, \&c.}$$

This equation will be true for any value of r and w, and consequently also for $w = o$, by which we have

$$a^{-r} + {}^1I^{-r}\ a^{-r-1}\ b + {}^2I^{-r}\ a^{-r-2}\ b^2, \&c. = \frac{1}{a^r + {}^1I^r\ a^{r-1}\ b + {}^2I^r\ a^{r-2}\ b^2, \&c.} = \frac{1}{(a+b)^r}{}^* = (a + b)^{-r},$$ and

consequently the truth of the theorem is also proved for any negative number.

ARTICLE IV.

Experiments on Prussic Acid. By M. Gay-Lussac.†

(Presented to the Institute, Sept. 18, 1815.)

THE experiments which I have the honour to communicate to the Class have for their object the nature of prussic acid and its combinations. Few bodies have been more studied, and yet few are less known. After the labours of Macquer, Scheele, and Berthollet, which form an epoch in the history of prussic acid, many other distinguished chemists made experiments upon it. I shall not however attempt to write a history of these; but merely notice the principal results which they have furnished, in order to point out the place from which I started.

* The case for a fractional exponent having already been proved.
† Translated from the Ann. de Chim. vol. xcv. p. 136.

It is to Macquer that we owe the first important experiments on Prussian blue.* On boiling it with a solution of potash in excess, that skilful chemist observed that nothing remained but oxide of iron, while the alkali combined with the colouring matter. On the other hand, if the Prussian blue predominate, the potash is completely saturated with the colouring matter, and loses its alkaline properties, just as if it were saturated with an acid. In both cases it has acquired the property of producing Prussian blue with solutions of iron, by means of double affinity; and it precipitates the greater number of the other metalline solutions. When Prussian blue is calcined, volatile alkali is formed—a fact which had likewise been observed by Geoffroy—and there remains oxide of iron attracted by the magnet, and a quantity of charcoal. From these experiments, Macquer concluded that Prussian blue is a compound of oxide of iron with an inflammable substance, which is converted by calcination into volatile alkali and charcoal.

Twenty years after, MM. de Morveau and Bergman considered the colouring matter as a particular acid, and the first gave it the name of *prussic acid*. However, its true nature remained still unknown.

Scheele, whose name is connected with so many brilliant discoveries, succeeded in 1782 in obtaining prussic acid in a separate state by a very ingenious process, and approached very near to a knowledge of the true nature of its constituents; for he ascertained that it is obtained by the union of ammonia with a charry matter rendered volatile by heat.†

To these important experiments of Scheele succeeded those of Berthollet, which are not less so.‡ He showed that Macquer's combination of the colouring matter and potash is a triple salt, of which iron constitutes the third element. On mixing chlorine with prussic acid, as obtained by Scheele, he observed that the first substance is changed into muriatic acid, and that the second has acquired a much stronger smell, and has lost part of its affinity for alkaline bases. In this new state it no longer forms Prussian blue with solutions of iron, but a green precipitate, which becomes blue when exposed to light, or when mixed with sulphurous acid. If potash be added, the prussic acid is completely destroyed, ammonia is produced, which is disengaged; and carbonic acid, which remains combined with the potash. From these results, and from the knowledge of the elements of ammonia, for which likewise we are indebted to Berthollet, he considered prussic acid as a compound of carbon, azote, and hydrogen. He does not admit oxygen into the number of its elements, and he supposes that the oxygen contained in the carbonic acid produced by the action of potash on prussic acid altered by chlorine, is furnished by this last substance. The absence of oxygen in prussic acid not being rigorously demon-

* Mem. de l'Acad. des Sciences, 1752.
† Opusc. de Scheele, vol. ii. p. 141.
‡ Ann. de Chim. vol. i. p. 30.

strated, several distinguished chemists have entertained doubts respecting its composition. Even Berthollet himself seems to partake of these; for he thus expresses himself in his Statique Chimique, vol. ii. p. 267: "These considerations do not constitute a rigorous proof of the absence of oxygen in prussic acid, and we ought to suspend our opinion on this subject, till pure prussic acid has been analyzed. The following observations even add to our uncertainty. However, in the explanation which I am going to give, I shall employ the hypothesis that it contains no oxygen in its composition."

From the analogy which subsists between prussic acid and sulphureted hydrogen, but particularly from the remark that the prussiates are decomposed by a heat not sufficient to form the alkaline ley of Prussian blue, M. Berthollet admits likewise (Stat. Chim. vol. ii. p. 267) that during the calcination of potash with animal matters, there is formed a combination of the alkali, carbon, and azote, which decomposes water when it comes in contact with it, and forms carbonic acid, ammonia, and prussic acid. This combination in fact takes place; but it is not demonstrated by the considerations employed by Berthollet; for the simple prussiate of potash is capable of bearing a very high temperature without losing the property of precipitating the solutions of iron blue; and we shall see that when the product of the calcination of potash and animal matters is dissolved in water, ammonia is only formed when it is thrown into the water when still red-hot.

Curaudau, without being acquainted with the work of Berthollet which was printed, but not published, when he read his memoir to the Institute, was led to the same opinion respecting the nature of the compound formed during the calcination of potash with animal matters. But his theory is so hypothetic, that I would take no notice of it, unless I were afraid that an hasty examination might find some resemblance between his opinions and a part of mine.

According to Curaudau, there exists a prussic radical, to which he gives the name of *prussine*, which is common prussic acid. This radical, by combining with oxygen, forms true prussic acid and its combinations, the prussiates. It acquires the neutralizing or acid properties only at the expense of the oxygen furnished by a metallic oxide, the presence of which is necessary to form with the acidifiable bases a strong and lasting compound. When an animal matter is calcined with potash, a compound is formed, which is nothing else than carbureted azote of potash. When dissolved in water, carbonic acid is produced at the expense of the oxygen of the water, and part of the carbon, while the hydrogen with the rest of the carbon and azote forms the prussine (Ann. de Chim. vol. xlvi. p. 148).

The most recent memoirs on prussic acid with which I am acquainted are those of Mr. Porrett, an extract of which may be found in the fourth and fifth volumes of Dr. Thomson's *Annals of Philosophy*. In the first Mr. Porrett treats of the triple prussiates. According to him, they contain, not prussic acid, but an unknown

acid, which he supposes is formed of carbon, azote, hydrogen, and black oxide of iron. He founds his opinion on this fact, that when a triple prussiate is exposed to the galvanic action, the alkali appears at the negative pole, while the oxide of iron and prussic acid appear at the positive pole.* In his second memoir Mr. Porrett gives the analysis of prussiate of mercury and prussic acid. I shall state this analysis hereafter. It is very different from mine, and I consider it as inaccurate.

In terminating this historical sketch, I must not forget the labours of Proust. I have made much use of them; and if I do not speak of them more at length here, it is because I shall have occasion frequently to quote them. I pass over a good many others in silence. But I mentioned already that it was not my intention to state every thing that is known respecting prussic acid.

That the observations which I have to present may assume a regular arrangement, I shall present them under four different articles. In the first I shall endeavour to explain the nature of prussic acid; in the second, I shall explain the properties of a new gas, which is the radical of prussic acid; in the third, I shall examine the combination to which the name of *oxyprussic acid* has been given; in the fourth, I shall treat of some prussiates. I regret that time did not permit me to carry that part of my labours further, but I hope to resume it afterwards.

I. *Of Prussic Acid.*

This acid may be obtained perfectly pure by the process which I described in the 77th volume of the Annales de Chimie, which consists in decomposing common prussiate of mercury by muriatic acid. The apparatus which I employ at present being somewhat simpler than my former one, I shall give a description of it. To the beak of a tubulated retort intended to hold the prussiate of mercury and muriatic acid, is adapted an horizontal tube, about six decimetres (two feet) long, and $1\frac{1}{2}$ centimetre (0·59 inch) in diameter in the inside. The first third part of the tube next the retort is filled with small pieces of white marble, to retain the muriatic acid that may come over, but which ought if possible to be prevented.† The two other thirds contain fused muriate of lime, likewise in small pieces, in order to condense the water which may be mixed with the prussic vapour. To the end of this tube is adapted a small receiver, destined to collect the acid. It must be surrounded with a frigorific mixture, or at least with ice, that the less acid may make its escape. The prussic acid is usually deposited upon the marble in the first portion of the tube. But by means of a moderate heat it may be made to pass successively through the whole tube,

* I need scarcely observe that this account of Mr. Porrett's paper is quite erroneous.—T.

† If muriatic acid pass into the tube, it will separate the carbonic acid of the marble, which, mixing with the vapour of prussic acid, will prevent its condensation, and occasion a considerable loss.

and after being left some time in contact with the muriate of lime, it may be finally driven into the receiver. I usually employ concentrated muriatic acid, and smaller in quantity than would be necessary to decompose the whole of the prussiate of mercury, and I reserve the residue to obtain an aqueous solution of prussic acid by adding a new quantity of muriatic acid.

Prussic acid, obtained by the method just described, possesses the following properties. It is a colourless liquid, having a strong smell, a taste at first cooling, then hot, asthenic in a high degree, and a true poison. Its specific gravity at $44\frac{1}{2}°$ is 0·7058; at 64°, it is 0·6969. It boils at $81\frac{1}{2}°$, and congeals at about 3°. It then crystallizes regularly, and affects sometimes the fibrous form of nitrate of ammonia. The cold which it produces when reduced into vapour, even at the temperature of 68°, is sufficient to congeal it. This phenomenon is easily produced by putting a small drop at the end of a slip of paper or a glass tube. Though I rectified it repeatedly on pounded marble, it retained the property of feebly reddening paper tinged blue with litmus. The red colour disappeared as the acid evaporated.

The specific gravity of its vapour compared to that of air is 0·9476. This is the mean of two experiments, differing but little from each other. I ascertained it by the method formerly described. By calculations founded on its composition, and the condensation of its elements, I obtained for its specific gravity only 0·9360, which is less than the preceding number by about a hundredth part. I think, however, that this last number should be preferred; for the difference between the two may be ascribed in part to errors in the experiment. This small density of the vapour of prussic acid, compared with its great volatility, furnishes a new proof that the density of vapours does not depend upon the boiling point of the liquids that furnish them, but upon their particular nature.

To appreciate the better the effects of prussic acid on other bodies, I began by determining exactly the nature and proportions of its elements. This acid being very volatile, I took advantage of the hot days of the month of August to analyze it in the eudiometer of Volta. My method was as follows.

I filled a glass jar about two-thirds with oxygen gas over a mercurial trough at the temperature of between 86° and 95°, and then filled it completely with the vapour of prussic acid. When the temperature of the mercury is reduced to that of the ambient air, I take a determinate volume of the gaseous mixture, and wash it in a solution of potash. The residue, compared with the absorption which has taken place, gives exactly the ratio of the oxygen to the prussic vapour. I can then employ this gaseous mixture without fearing that the prussic acid will condense, provided the temperature be not too low; but during my experiments it was never under $71\frac{1}{2}°$. I introduce a known volume into Volta's eudiometer, all the wires of which are of platinum, and I pass an electric spark through it. The combustion is very lively, and of a bluish-white colour. A white

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prussic vapour accompanies it, and a diminution of volume takes place, which is ascertained by measuring the residue in a graduated tube. This residue, washed with a solution of potash or barytes, experiences a new diminution, owing to the absorption of the carbonic acid gas formed. Lastly, the gas which the alkali has left is analyzed over water by hydrogen, and it is ascertained to be a mixture of azote and oxygen, because this last gas was employed in excess.

The white vapour of which I have spoken appeared to me owing to a little nitric acid and vapour of water formed by the combustion; for when a little water was introduced into the eudiometer, after several detonations, it became muddy, letting fall oxide of mercury, and reddened litmus.

Supposing we were to operate upon a gaseous mixture containing 100 of prussic acid vapour, we obtain the following results, which are the mean of four experiments:—

Vapour	100·0
Diminution after combustion	78·5
Carbonic acid gas produced	101·0
Azotic gas	46·0
Hydrogen gas	55·0

During the combustion a quantity of oxygen disappears equal to about $1\frac{1}{4}$ of the vapour employed. The carbonic acid gas produced represents one volume, and I suppose that the other fourth is employed in forming water; for it is impossible to doubt that hydrogen enters into the composition of prussic acid. From the laws of chemical proportions, we may conclude that prussic vapour contains just as much carbon as will form its own bulk of carbonic acid, half a volume of azote, and half a volume of hydrogen. This result is evident for the carbon; and though, instead of 50 azote and hydrogen, which ought to be the numbers according to our supposition, we obtain 46 for the first, and 55 for the second. This is doubtless owing to a portion of the azote and oxygen having disappeared, in order to form nitric acid. On this supposition we ought obviously to find too little azote and too much hydrogen, because the quantity of this last can only be judged of by the oxygen which has disappeared. But are the elements which I have pointed out the only ones which enter into the composition of prussic acid? Are the proportions exact? We shall answer these questions by comparing the density of prussic vapour to the sum of that of its elements; and by attending to this, that since a volume of vapour produces a volume of carbonic acid gas, half a volume of azote, and half a volume of hydrogen, the density of the vapour, if our analysis be correct, ought to be equal to that of the vapour of carbon, and to half that of azote and hydrogen.

But the density of carbonic acid gas being 1·5196, and that of oxygen 1·1036, the density of the vapour of carbon is 1·5196 − 1·1036 = 0·4160.

Half a volume of hydrogen	=	0·0366
Half a volume of azote	=	0·4845
Total		0·9371

Thus, according to the preceding analysis, the density of prussic vapour is 0·9371, and I found it by direct experiment 0·9476. Notwithstanding this difference of 0·01 which exists between these two numbers, and which may be an error in the experiment, I think that we ought to consider it as demonstrated that prussic vapour contains one volume of the vapour of carbon, half a volume of azote, and half a volume of hydrogen, condensed into one volume, and that no other substance enters into its composition.

It is now easy to explain the diminution of volume produced by the electric spark in a mixture of prussic vapour and oxygen. It ought to be equal to $1\frac{1}{4}$ volume of vapour, since that quantity of oxygen has disappeared. But half a volume of azote becomes free. This reduces the apparent diminution to three-fourths of a volume of the prussic vapour, or to 75 per cent. I found it 78·5. But as it appears demonstrated that azote and oxygen disappear during the combustion of the prussic vapour, to form nitric acid, this affords a sufficient explanation of the excess in the diminution. That this excess is not exactly proportional to the azote and oxygen which have disappeared, must be ascribed to the unavoidable errors of experiment.

The nature of prussic acid appears to me, then, perfectly known. But if any doubts remain, they will be removed by the following analysis.

I made about two grammes (30·88 grains) of prussic vapour pass slowly through a red-hot porcelain tube over 0·806 gramme (12·45 grains) of harpsicord wire rolled in the form of a very short cylinder. I obtained two products, a gaseous mixture composed of equal volumes of azote and hydrogen, and charcoal, a part of which was deposited on the iron, and another intimately combined with it. The gaseous mixture contained no carbon; for after its detonation with oxygen, potash produced no diminution in it. I observed that the charcoal was deposited only in that part of the tube which contained the iron, though it occupied only a small portion of it, and though the prussic vapour experienced a very high temperature before coming to the iron. It is true that the carbon, being united to the iron, we may ascribe the decomposition of the acid to the affinity of this metal for carbon; but as there is a great quantity which merely adheres to the metal, this explanation is not sufficient. Prussic vapour appears to me to act the same part as ammonia, which, according to the curious remark of Thenard, supports a very high temperature in a porcelain tube without being decomposed, and which is decomposed with the greatest facility even at a much lower temperature, when it comes in contact of a metal, to which, however, it communicates nothing.

The iron in our experiment had become very brittle. Being combined with charcoal, and entirely surrounded with it, there was no probability that it contained oxygen. But to decide the point, I dissolved it in muriatic acid, comparatively with a determinate weight of the same iron wire. I obtained a volume of hydrogen gas equal to $\frac{101}{114}$ of that which the same wire would have given in a state of pure iron. There remained a quantity of carburet of iron weighing 0·155 gramme (2·39 grains). This, being calcined with red oxide of mercury, was reduced to 0·076 gramme (1·17 grain). This quantity of oxide represents $\frac{8}{114}$ of hydrogen. Thus I obtained $\frac{109}{114}$. The loss $\frac{5}{114}$ is too small to indicate the presence of oxygen in prussic acid. It may be very well ascribed to the experiment.

These results appear to me to demonstrate that prussic acid contains equal volumes of hydrogen and azote, and that it contains no oxygen. To determine the quantity of carbon combined with these two bodies, I passed prussic vapour over the brown oxide of copper almost at a red heat. The vapour was entirely decomposed, the copper was reduced, and drops of water appeared in the tube. The gases disengaged, and which were collected over mercury, were a mixture of two parts carbonic acid and one part azotic gas. This result, taken along with the preceding, demonstrates the nature of prussic acid, and confirms the analysis of it made by the eudiometer. This process, which I employed only after the first analysis, is so simple that it may be exhibited even in a lecture.

Thus from these analyses it appears evident that prussic acid is composed of

One volume of the vapour of carbon.
Half a volume of hydrogen.
Half a volume of azote.

condensed into one volume; or in weight of

Carbon	44·39
Azote	51·71
Hydrogen	3·90
	100·00

This acid, when compared with other animal substances, is distinguished by the great quantity of azote which it contains, by its small quantity of hydrogen, and especially by the absence of oxygen. Its acid properties cannot depend upon the hydrogen, which is very alkalifying, but upon the carbon and azote. We ought to consider it as a true hydracid, in which the carbon and azote supply the place of the chlorine in muriatic acid, the iodine in hydriodic acid, and the sulphur in sulphureted hydrogen; but this assertion requires a fuller elucidation.

I have likewise attempted to decompose prussic vapour mixed with hydrogen by means of electricity. After having passed through it at least 50,000 sparks, all the vapour was not decomposed, and that which was had more than doubled its volume. The platinum

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wires, and the portion of the tube through which the spark passed, was covered by a slight bistre-coloured coating, showing that carbon had been precipitated, or at least a very carbonaceous combination. On analyzing the gas, I obtained in fact a little less carbon than the calculation indicated. As to the azote and hydrogen, I found them nearly in the same proportion as in the preceding analyses. However, this experiment, not having given me satisfactory results, after having twice repeated it, and being very tedious, I did not think it worth while to persist in it any longer.

The analysis of prussic acid which I have given ought to precede the examination of its action on other bodies. This examination will not any longer present any difficulty; but before undertaking it, I must remark that my results are quite different from those obtained by Mr. Porrett; for, according to him, prussic acid is composed of

Carbon	24·8
Azote	40·7
Hydrogen	34·5
	100·0

But, according to my analysis, of

Carbon	44·39
Azote	51·71
Hydrogen	3·90
	100·00

Not knowing his memoir but by the very concise extract of it given by Dr. Thomson, it is not in my power to explain the cause of this great difference; but it is evident that the proportion of hydrogen which he gives is too great.*

In examining the properties of prussic acid, I shall not restrict myself to a rigorous arrangement. I shall state my experiments in the order in which they will throw light on each other. Prussic acid containing three elements, ought necessarily to possess great mobi-

* Mr. Porrett's analysis was made by heating a mixture of prussiate of mercury and red oxide of mercury. He obtained one volume of carbonic acid gas and half a volume of azotic gas; and he inferred the quantity of hydrogen from the oxygen which had disappeared over and above what was necessary for the formation of the carbonic acid. His result was as follows:—

Carbon	34·8
Azote	40·7
Hydrogen	24·5
	100·0

The proportions of carbon and azote are the same as those given by Gay-Lussac. The hydrogen is too great, because Mr. Porrett supposed the mercury in prussiate of mercury was in the state of oxide. This greatly increased the supposed quantity of oxygen consumed. In every thing, except this wrong inference, Mr. Porrett's analysis is equally correct with that of Gay-Lussac. Mr. Porrett found five times the quantity of red oxide of mercury in the prussiate necessary to decompose the whole acid of the prussiate.—T.

lity. To form an idea of its constitution, we may compare it to sulphureted hydrogen. But this mobility is only relative; it depends on the circumstances in which the acid is placed.

When this acid is kept in well-closed vessels, even though no air be present, it is sometimes decomposed in less than an hour. I have often kept it 15 days without alteration; but it is seldom that it can be kept longer, without exhibiting signs of decomposition. It begins by assuming a reddish-brown colour, which becomes deeper and deeper, and it gradually deposites a considerable carbonaceous matter, which gives a deep colour both to water and acids, and gives out a strong smell of ammonia. If the bottle containing the prussic acid be not hermetically sealed, nothing remains but a dry charry mass, which gives no colour to water.

To know exactly the results of this decomposition, I introduced prussic acid into a barometrical tube well freed from air, and waited till the inside of the tube was coated with a charry covering, which rendered it opake. The height at which the mercury stood was inconsiderable; but on inclining the tube, the mercury filled it, which shows that no gas was extricated. On lifting up the tube, I recognized the odour of prussic acid. Water introduced acquired a strong brown colour. Potash and lime disengaged ammonia from it, and sulphuric acid rendered the odour of prussic acid very sensible; but no carbonic acid was disengaged. From this it is evident that by the decomposition of prussic acid a portion of ammonia is formed, which combines with the remaining prussic acid. The charry substance must of necessity contain a quantity of azote; for ammonia being composed of three volumes of hydrogen and one of azote, while prussic acid contains equal volumes of these two elements, two-thirds of the azote must remain with the carbon, and form of consequence an azoturet of carbon.

Phosphorus and iodine, being volatilized in prussic vapour, did not appear to produce any alteration. Sulphur treated in the same way absorbs it readily. We obtain a solid compound of sulphur and prussic acid, which I consider of the same nature as that formed by sulphureted hydrogen and the radical of prussic acid, of which I shall speak afterwards. I postpone, likewise, the examination of the compound formed by chlorine and prussic acid.

Among the simple metallic bodies, potassium is one of those whose action is most proper to throw light on the true nature of prussic acid. When heated in prussic vapour mixed with hydrogen or azote, there is absorption without inflammation, and the metal is converted into a grey spongy substance, which melts, and assumes a yellow colour. Supposing the quantity of potassium employed capable of disengaging from water a volume of hydrogen equal to 50 parts, we find, after the action of the potassium,

1. That the gaseous mixture has experienced a diminution of volume amounting to 50 parts:

2. On treating this mixture with potash, and analyzing the residue by oxygen, that 50 parts of hydrogen have been produced:

3. And consequently that the potassium has absorbed 100 parts of prussic vapour; for there is a diminution of 50 parts, which would obviously have been twice as great had not 50 parts of hydrogen been disengaged.

When the yellow matter is put into water, it dissolves entirely, without the least effervescence, and exhibits all the characters of simple prussiate of potash obtained by combining directly the acid and alkali. If we suppose the water to be decomposed, which is very probable, but which must necessarily happen by the joint action of an acid, the potassium combines with its oxygen, and the hydrogen, which is precisely equal to that which the potassium disengaged from the prussic acid, reproduces this acid with all its properties.

Here, then, is a very great analogy between prussic acid and muriatic and hydriodic acids. Like them, it contains half its volume of hydrogen; and, like them, it contains a radical which combines with the potassium, and forms a compound quite analogous to the chloride and iodide of potassium. The only difference is, that this radical is compound, while those of the chloride and iodide are simple.

Since prussic acid contains

One volume of vapour of carbon.
Half a volume of azote.
Half a volume of hydrogen.

And since I have just proved that potassium disengages half its volume of hydrogen, it is obvious that the substance which combines with the metal, and which ought to be distinguished by the name of prussic radical, is a compound of carbon and azote, in the proportion of

One volume of vapour of carbon.
Half a volume of azotic gas.

This radical combined with potassium forms a true prusside of that metal. We ought, therefore, to consider prussic acid as a *hydracid*; and with the less hesitation, that a great number of other facts lead to the same conclusion.

The name *prussic acid*, then, will no longer suit it; but it must be called *hydro-prussic*. We must likewise invent a name for its radical, from which this may be derived. Were we to preserve the term *prussic*, which has never been adopted in Germany, and which never can be, we should be obliged to give it a meaning different from that which it has hitherto borne. These considerations have induced me to invent a new name for the radical of prussic acid. That of *cyanogen** having appeared very proper to the chemists of this capital, I have adopted it, and shall use it afterwards in the course of this memoir. Common prussic acid will receive the name of *hydro-cyanic acid*, and the prussiates that of *hydro-cyanates*. The combinations of cyanogen with simple bodies, when it performs

* From κύανος, blue; αγεννάω, I produce.

the same part as chlorine in the chlorides, will be denoted by the term *cyanuret.** It will be difficult to give the prussic radical a more convenient name; for it will be seen that it acts at once the part of a compound and simple body; and if we wished to denote it by the name *carburet of azote*, which would suit it as a compound body, circumlocutions would be necessary to denote its numerous compounds. I return now to the properties of the combination of cyanogen and potassium, or of the cyanuret of potassium.

Its solution in water is very alkaline, even when a quantity of hydro-cyanic vapour is employed much greater than the potassium is able to absorb. Yet the chloride and iodide of that metal are perfectly neutral. This very remarkable difference, depending doubtless on the peculiar disposition of the molecules, does not exist with regard to sulphur. I heated in sulphureted hydrogen a quantity of potassium which would have disengaged 50 parts of hydrogen from water; and I withdrew the sulphuret from the action of the gas as soon as the combustion was completed. The diminution of volume was 50 parts, and the residue, treated by potash, left 50 parts of hydrogen; so that the potassium had combined with 50 parts of sulphureted hydrogen, and it had decomposed 50, of which it had seized the sulphur, and left the hydrogen.

This combination of sulphureted hydrogen and sulphuret of potassium, in which this last substance seems to act the part of the oxides in the salts, dissolves in water without effervescence, and renders it alkaline. The sulphureted hydrogen had not rendered the water muddy by its decomposition. Thus, sulphur and cyanogen present this analogy, that both of them form alkaline combinations with potassium.†

Knowing the composition of hydro-cyanic acid, and that potassium separates from it as much hydrogen as from water, it is easy to find the proportional number which represents the capacity of this acid, as well as that which represents that of cyanogen, the capacity of oxygen being 10; for we must take such a quantity of hydro-cyanic acid that its hydrogen is capable of saturating 10 of oxygen. In this manner we shall find the proportional number for this acid 33·846; and subtracting from this number the weight of the hydrogen, there remains 32·520, which is the proportional number for cyanogen.

A high temperature produces a very remarkable alteration in hydro-cyanic acid. On passing its vapour through a porcelain tube, we obtain hydrogen, a little azote, and cyanogen, mixed with a considerable portion of the acid not decomposed; and the inside of

* The term *cyanide* would be better.—T.

† I considered the part similar to the oxides which the sulphurets play in certain combinations analogous to the salts in the memoir which I read last year to the Philomatic Society, but which is not yet published. In it I gave the analysis of sulphureted sulphite of strontian, in which the sulphur converted into sulphuric acid is capable of saturating a quantity of base double that in the sulphite; from which I conclude that, setting out from the sulphite, we may increase the quantity of oxygen or of sulphur without altering its neutrality.

the tube is covered with a slight coating of charcoal. This decomposition is similar to that which sulphureted hydrogen undergoes; for Cluzel has shown that this last is partly converted into hydrogen and sulphur by heat.

It will be recollected that iron at a red heat decomposes hydro-cyanic acid. The elastic fluid collected is a mixture of equal volumes of azote and hydrogen. The greatest part of the carbon is deposited round the iron, and a small part combines with it. Copper and arsenic have no action on hydro-cyanic acid. Platinum appears to decompose it at a high temperature, but the result is the same as is produced by heat alone.

The oxides produce on hydro-cyanic acid a variable action depending on their affinity for oxygen.

Having placed barytes, recently prepared from its nitrate, in a glass tube heated to an obscure red, and made hydro-cyanic vapour pass over its surface, the barytes became slightly incandescent. It became soft, and then dried. No water was disengaged, but only pure hydrogen gas.*

This experiment shows that barytes decomposes hydro-cyanic vapour in a manner analogous to that in which it decomposes muriatic acid gas. But we obtain hydrogen in the first case, and water in the second, in consequence of the difference in the affinities of barium for cyanogen and chlorine.

Since hydro-cyanic acid in combining with barytes loses its hydrogen, the compound is a true cyanuret of barytes; when placed in contact with water, it ought to produce compounds analogous to the chlorates, iodates, or sulphites; that is to say, containing an acid composed of oxygen and cyanogen, which would be *cyanic acid*, strictly so called. But there is here a peculiar circumstance, which modifies a great deal the results; namely, cyanogen is a compound, and its elementary affinities appear more energetic than its resulting affinities. It is certain, at least, that by dissolving a cyanuret in water, we do not form a combination of oxygen and cyanogen. I shall soon examine this subject more particularly.

Instead of barytes, potash prepared by means of alcohol may be employed. The experiment may be made in a small curved glass tube, and it is more easy than the preceding one. Cyanuret of potash is obtained, and hydrogen is disengaged; but its quantity is greater than the hydro-cyanic acid could furnish, because the water which the potash contains contributes to the decomposition of a part of the cyanogen.

I likewise formed cyanuret of soda by passing hydro-cyanic vapour over dry carbonate of soda in a glass tube heated obscurely red-hot. The acid of the carbonate is disengaged, and an inflammable gas is obtained, which is not pure hydrogen, because both this gas and

* This experiment may be conveniently made with hydro-cyanic vapour mixed with azote or hydrogen in a small bent tube, heated by a spirit of wine lamp; but the incandescence will not be seen, as the absorption will be less rapid. While cold, barytes exercises no sensible action on hydro-cyanic vapour.

hydro-cyanic vapour are capable of acting upon carbonic acid at a high temperature, and of partly decomposing it.

I have already shown that the oxide of copper decomposes hydro-cyanic acid completely at a red heat, and that water is obtained, and a mixture of one volume carbonic acid and half a volume of azote. But I wished to ascertain if the action of these two bodies would not be different at the common temperature. I accordingly introduced peroxide of copper into a tube with hydro-cyanic vapour mixed with hydrogen. The absorption took place by degrees; but was not so great as it would have been if all the vapour had been destroyed. Having turned up the tube to ascertain by the smell if any hydro-cyanic acid remained, I observed with surprise that cyanogen had been formed, easily recognizable by the strong and penetrating odour which characterizes it. On exposing the oxide to a gentle heat, a good deal of water separated from it; so that it appears that hydro-cyanic acid is acted upon in the same manner by peroxide of copper as muriatic acid by peroxide of manganese. On putting oxide of copper into the liquid acid, diluted with water, the smell of cyanogen became very sensible in a few days, and the oxide became white on the surface. The peroxide of manganese absorbs completely hydro-cyanic vapour in a few hours. Water is formed; but cyanogen does not become sensible. I shall examine more particularly hereafter what takes place on this occasion.

The red oxide of mercury, when assisted by heat, acts so powerfully on hydro-cyanic vapour, that the compound which ought to be formed is destroyed by the heat disengaged. The same thing happens when a little of the concentrated acid is poured upon the oxide. A great elevation of temperature takes place, which would occasion a dangerous explosion if the experiment were made upon considerable quantities. When the acid is diluted, the oxide dissolves rapidly, with a considerable heat, and without the disengagement of any gas. Nothing is obtained but the substance formerly called *prussiate of mercury*.

When the oxide is placed in contact with the vapour of hydro-cyanic acid, mixed with hydrogen, without applying heat, the vapour is absorbed in a few minutes. On emptying the tube of the hydrogen in order to fill it with a new mixture, that the result might be the more sensible, the absorption of the vapour was as complete as the first time, and the hydrogen remained with the volume which it ought to have had, which shows that it had no part in the phenomenon. After some similar operations, the oxide adhered to the sides of the tube. Having collected it at the bottom of the tube, and applied a gentle heat, a good deal of water was evaporated.

Hence when the peroxide of mercury acts in the cold on hydro-cyanic acid, the oxygen of the first combines with the hydrogen of the second, which by this last is reduced to its radical. We ought, therefore, to obtain, not hydro-cyanate of mercury, but cyanuret of mercury. Common prussiate of mercury, which is exactly the same, must likewise bear the same name.

The red oxide of mercury absorbing hydro-cyanic vapour with so much facility, I point it out as very proper to separate it from most of the gases with which it may be mixed. I have used it several times with success.

From these few experiments, we see that the oxides produce different effects on hydro-cyanic vapour. Those in which the oxygen is strongly condensed disengage the hydrogen, and form cyanurets of the oxide; but the oxides in which the oxygen is weakly condensed act upon it in so many ways that they require to be more accurately examined than I have done to obtain any general results.

(To be continued.)

Article V.

Meteorological Table: extracted from the Register kept at Rose-Bank, Perth. Latitude, 56° 25′. *Elevation,* 130 *feet.* By a Young Gentleman of the Perth Academy.

The last column of the annexed table (Pl. XLVIII.) contains, under the head Thermometer, the mean temperature of water from a well 25 feet deep, taken three times every month, viz. on the 5th, 15th, and 25th. The extreme temperatures observed were 42·4° on the 5th and 15th of March, and 48·8° on the 25th of October and the 5th of November, being an annual variation of 6·4°. This variation may be easily accounted for on the commonly received theory of the origin of springs, of which it affords in its turn a beautiful illustration. The temperature of the atmosphere reaches its maximum about the end of July, and its minimum about the end of January. But as the influence of the sun's rays in the one case, and the intensity of the frost in the other, cannot be supposed to penetrate to such a depth as directly to affect the temperature of water 25 feet below the surface, the change in that temperature as indicated in the table must have taken place at or near the surface of the ground. This leads naturally and directly to the true theory of springs and wells, viz. that the water which is deposited on the higher grounds from the atmosphere descends through the earth as in a filter, till, being arrested by an impermeable stratum, it flows along the surface of that stratum, and either bursts out in springs, or is intercepted by pits dug for the purpose; and this theory being admitted, it is easy to deduce from it the law of variation in the temperature of pump water. The rain and melted snow of winter, being cooled down on their first entering the ground, far below the mean temperature of the interior of the globe, successively abstract from the strata through which they pass a portion of caloric; and though, from the quantity of water bearing so small a proportion to the body of earth through which it passes, that body cannot be re-

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P. M.
Dry.
5·5
10·2
18·4
30·5
30 6
38 0
38·0
34·3
27·9
14·5
9·9
7·2
22·1

Lowest.	
Tm.	Pre
45	28 9
50	29·1
47	28·6
52	29·3
60	29·3
60	29·3
65	29·3
65	29·3
57	29·2
56	29·0
53	28·5
47	28 6

METEOROLOGICAL TABLE.

1815.	THERMOMETER. Mean of daily Extremes. Max.	Mean of daily Extremes. Min.	Mean at 10 o'clock daily. A.M.	Mean at 10 o'clock daily. P.M.	Mean of the Max. and Min.	Mean of Ten and Ten.	Mean of the whole.	Pump water. Mean tm. 3 ob. monthly.	BAROMETER. Mean of daily observation. 10 A.M. Tem.	10 A.M. Press.	10 P.M. Tem.	10 P.M. Press.	Mean of both observations. Tem.	Mean of both observations. Press.
January	35·3	28·5	32·2	31·6	31·9	31·9	31·9	43·40	44	29·812	44	29·805	44	29·808
February	43·6	35·7	40·3	40·2	39·6	40·3	39·9	42·70	50	29·551	50	29·553	50	29·552
March	45·3	34·9	41·3	39·4	40·1	40·4	40·2	42·47	51	29·396	51	29·419	51	29·407
April	52·2	36·9	46·8	42·6	44·6	41·7	44·6	43·07	55	29·822	55	29·847	55	29·834
May	59·3	45·5	54·0	49·2	52·4	51·6	52·0	44·50	59	29·716	59	29·767	59	29·741
June	61·5	48·9	59·0	53·1	56·7	56·0	56·4	45·40	62	29·789	62	29·799	62	29·794
July	66·5	50·3	60·9	54·9	58·4	57·9	58·1	46·07	64	29·971	64	29·981	64	29·977
August	64·8	50·9	59·7	55·7	57·8	57·7	57·8	46·80	63	29·713	63	29·70[illegible]	63	29·707
September	59·8	46·9	55·7	50·7	53·4	53·2	53·3	47·43	60	29·797	60	29·789	60	29·793
October	51·7	42·6	48·5	46·3	47·2	47·1	47·3	48·10	55	29·690	56	29·712	56	29·701
November	40·8	31·4	36·6	36·6	36·1	36·6	36·3	48·42	49	29·791	49	29·786	49	29·788
December	35·7	27·8	32·3	31·9	31·8	32·1	32·0	46·17	45	29·600	46	29·599	46	29·599
Ann. Results.	51·6	40·0	47·3	44·3	45·8	45·8	45·8	45·38	55	29·721	55	29·730	55	29·725

1815.	HYGROMETER. LESLIE'S. Mean of daily observation. 10 A.M. Tem.	10 A.M. Dry.	4 P.M. Tem.	4 P.M. Dry.	10 P.M. Tem.	10 P.M. Dry.	Mean of the whole. Tem.	Mean of the whole. Dry.	RAIN. Monthly amount. In. Dec.	RAIN. Daily mean. In. Dec.	EVAPORATION. Monthly amount. In. Dec.	EVAPORATION. Daily mean. In. Dec.	WEATHER. Fair days or rain less than ·01.	Wet days.	WIND. Prevailing direction. N and N E	E and S E	S and S W	W and N W
January	33	5·5	34	5·5	31	3·8	33	4·9	0·945	0·030	0·265	0·008	20	11	2	9	1	19
February	42	8·6	43	10·2	40	5·4	42	8·1	2·007	0·072	0·377	0·013	12	16	0	8	5	15
March	44	16·8	45	18·4	39	8·9	43	14·7	1·990	0·064	0·667	0·021	13	18	0	5	3	23
April	49	24·6	51	30·5	42	14·1	47	23·1	0·999	0·033	1·373	0·045	22	8	8	6	1	15
May	56	25·6	57	30·6	49	12·8	54	23·0	2·334	0·075	1·597	0·051	16	15	0	12	5	14
June	62	30·8	62	38·0	53	14·2	59	29·7	0·871	0·029	1·483	0·049	21	9	0	10	5	15
July	64	40·4	63	38·0	55	17·5	61	32·0	1·743	0·056	1·945	0·063	16	15	0	8	6	17
August	62	32·1	63	34·3	55	16·8	60	27·7	1·324	0·042	1·346	0·043	23	8	0	7	3	21
September	57	23·3	58	27·9	51	9·7	55	20·3	2·193	0·073	1·125	0·037	14	16	0	4	16	10
October	50	14·2	50	14·5	46	7·7	49	12·2	3·362	0·108	0·888	0·028	16	15	4	14	12	1
November	38	8·6	39	9·9	36	4·8	38	7·8	1·643	0·054	0·390	0·013	24	6	3	6	5	16
December	33	7·7	34	7·2	32	4·6	33	6·5	1·343	0·043	0·260	0·008	22	9	0	10	3	18
Ann. Results.	49	20·3	50	22·1	44	10·0	48	17·5	20·754	0·056	11·716	0·032	219	146	17	99	65	184

MONTHLY EXTREMES.

1815.	THERMOMETER. Extremes in 24 hours. Max. Dt.	Max. Tm.	Min. Dt.	Min. Tm.	Extremes at 10 o'clock. Morning. Highest. Dt.	Morning. Highest. Tm.	Morning. Lowest. Dt.	Morning. Lowest. Tm.	Evening. Highest. Dt.	Evening. Highest. Tm.	Evening. Lowest. Dt.	Evening. Lowest. Tm.
January	9	44·0	22	11·0	3	41·0	23	17·0	2	43·0	23	13·0
February	22	51·0	9	26·0	25	48·5	10	31·0	21	48·0	9	31·0
March	31	53·5	11	22·5	31	49·0	12	32·0	26	48·0	11	30·0
April	28	62·0	14	31·0	28	54·0	14	37·0	11	52·0	14	34·0
May	26	69·0	31	39·0	19	63·0	1	46·0	19	55·0	21	43·0
June	30	75·5	6	41·5	27	66·0	20	53·0	27	62·0	6	48·0
July	12	72·0	6	43·5	25	66·5	8	55·5	16	61·0	3	49·0
August	2	70·0	21	43·0	2	65·0	19	54·0	1	62·0	6	50·0
September	14	69·0	22	34·0	1	63·5	28	47·0	9	59·0	22	42·0
October	8	57·5	30	30·0	4	54·0	31	35·0	3	55·0	31	35·0
November	9	56·5	24	18·0	9	55·0	25	21·5	9	51·0	24	23·0
December	12	45·0	19	7·5	1	39·0	17	18·0	31	42·0	19	7·5

1815.	BAROMETER. Extremes at 10 o'clock. Morning. Highest. Dt.	Morning. Highest. Tm.	Morning. Highest. Press.	Morning. Lowest. Dt.	Morning. Lowest. Tm.	Morning. Lowest. Press.	Evening. Highest. Dt.	Evening. Highest. Tm.	Evening. Highest. Press.	Evening. Lowest. Dt.	Evening. Lowest. Tm.	Evening. Lowest. Press.
January	17	46	30·391	29	44	29·053	17	43	30·381	10	45	28·903
February	28	53	30·327	4	49	29·011	27	53	30·281	16	50	29·151
March	2	51	30·241	13	48	28·629	1	53	30·150	12	47	28·625
April	18	55	30·341	21	55	29·181	17	54	30·321	21	52	29·306
May	26	60	30·085	12	62	29·170	26	62	30·125	20	60	29·340
June	29	66	30·280	6	60	29·400	28	66	30·285	6	60	29·395
July	2	62	30·245	18	63	29·470	1	62	30·290	17	65	29·350
August	1	65	30·231	19	60	29·355	1	67	30·215	5	65	29·375
September	7	57	30·163	26	59	29·170	7	59	30·150	30	57	29·200
October	29	50	30·337	20	55	28·825	8	59	30·392	19	56	29·025
November	26	45	30·586	13	54	28·415	25	45	30·608	13	53	28·525
December	10	46	30·398	24	45	28·745	10	45	30·360	16	47	28·605

1815.	HYGROMETER. Extremes observed. 10 Morning. Highest. Dt.	10 Morning. Highest. Tm.	10 Morning. Highest. Dr.	10 Morning. Lowest. Dt.	10 Morning. Lowest. Tm.	10 Morning. Lowest. Dr.	4 Afternoon. Highest. Dt.	4 Afternoon. Highest. Tm.	4 Afternoon. Highest. Dr.	4 Afternoon. Lowest. Dt.	4 Afternoon. Lowest. Tm.	4 Afternoon. Lowest. Dr.	10 Evening. Highest. Dt.	10 Evening. Highest. Tm.	10 Evening. Highest. Dr.	10 Evening. Lowest. Dt.	10 Evening. Lowest. Tm.	10 Evening. Lowest. Dr.
January	17	38	20	1	31	0	17	36	20	2	32	0	11	34	15	1	37	0
February	22	52	18	1	38	0	28	49	24	1	37	1	26	37	14	1	45	0
March	16	45	38	15	38	4	18	48	37	15	40	4	17	41	23	12	33	2
April	25	51	52	30	43	5	25	54	64	13	40	2	18	44	24	30	43	3
May	19	64	49	30	50	0	26	71	64	29	49	6	14	50	26	28	49	2
June	2	67	60	19	54	5	8	64	60	11	57	5	30	59	36	13	50	0
July	14	68	62	15	59	15	14	68	60	29	58	0	7	51	30	19	52	2
August	8	65	67	23	61	10	2	72	61	23	65	10	13	58	30	23	58	5
September	6	54	43	29	51	5	6	58	51	29	54	0	4	50	30	16	55	0
October	11	54	34	20	46	0	10	53	30	20	46	0	12	47	23	31	35	0
November	9	57	25	25	21	0	11	48	22	28	34	0	13	38	16	30	38	0
December	30	40	22	2	38	0	13	40	20	2	41	0	13	38	15	2	38	0

duced more than a few degrees below its mean temperature, yet it is obvious that the diminution must continue till the surface again approaches the temperature of the interior. This equilibrium will take place towards the middle of March, as the surface of the earth is generally within a few degrees of the mean annual temperature. It is found accordingly, as might be expected, that the temperature of the interior as indicated by the pump water is actually a minimum about the middle of March. From that period it gradually increases, and appears to reach its mean about the middle of June. The rain and dews, however, of the succeeding months, being still at a comparatively high temperature, communicate additional caloric to the strata beneath, and must continue to do so till the surface of the ground again descend towards the temperature to which the interior has risen. This point, for the reason already mentioned, will be a few degrees above the mean, and of course the equilibrium ought to take place about the beginning of October, as the temperature of the atmosphere is then generally within a few degrees of the mean annual temperature. It is obvious, however, that the ground, to the depth of several feet, may, from the accumulation of the sun's rays, be preserved at a higher temperature than the mean, even after that of the atmosphere has sunk considerably lower. This will happen perhaps to a certain extent every year, but especially in warm and dry seasons. Making an allowance, therefore, for this circumstance, the equilibrium between the surface and the interior may be expected to take place about the *end* of October, which agrees exactly with observation, the pump water being then a maximum. From this period the temperature decreases, and reaches its medium again towards the end of December. Besides the coincidence between the above theory and the *general results* of observation, it is further confirmed by several circumstances of a more particular nature. During the whole of the month of August (1815), when the rain that fell was wholly consumed by evaporation, and of course could contribute nothing to springs, the temperature of the well was stationary at 46·8°. In the course of September, and the first 15 days of October, when the quantity of rain considerably exceeded the evaporation, the temperature rose to 47·8°, being 1° in an interval of about 50 days; and between the 15th and 25th of October, during which time there fell upwards of two inches of rain, with little evaporation, it rose to 48·8°, being 1° in about one-fifth of the preceding interval. It may be inferred that, had the last fall of rain taken place sooner, the pump water would have sooner reached its maximum. But might not the truth of the whole theory be put to the test of experiment, by noting the temperature and quantity of rain, ascertaining as nearly as possible the density and depth of the strata through which it passes before being conveyed to the pump, and determining by calculation the quantity of caloric which it ought to abstract from, or communicate to, these strata? And if

the theory be found to hold in all cases, modified perhaps by the nature of the ground, and other circumstances, will not some correction be necessary in determining the temperature of any place from a single observation of spring water, unless when that observation has been made about the middle of winter or the middle of summer?

Rose-Bank, March 9, 1816.

Article VI.

Description of the Bird Fly. By M. W. Carolan.

(To Dr. Thomson.)

Sir,

The history of those insects which infest other animals is a curious subject of investigation, and includes no small branch of natural history. Amongst the varieties of this description, there are few which exhibit a more striking fitness for their situation than the fly which lives upon some birds, and may be often observed upon the partridge and swallow. This fly, like most others, seems to live principally upon animal perspiration, or any thing in a putrescent state; but as it could not get at the cuticular pores of birds without going through the feathers, it is exactly formed for that purpose.

It is rather larger than the common fly, of a clear tea-green colour, and is seldom seen but upon birds. It is quite flat, so that both its body, head, and legs, apply closely to any plain surface on which it may rest. It is very hard, and not easily crushed or killed. Its legs are very strong, and it can move in all directions; and, what is curious, runs with most rapidity sidewise, and does not seem to run easily straight forward. Its flatness, strength, and polished smoothness, without any hairs upon its body, enable it to move with ease among the feathers, and particularly its capacity of running sidewise, which gives it the power of going round the body of the bird beneath the feathers; for the feathers of birds are so placed in transverse rings that no insect, except it were almost invisible, could go straight forward under them. It sometimes appears above the plumage to enter in at a new place, which it performs with great ease and quickness, and without discomposing a single reed of the feathers.

These may seem necessary to preserve the health of some animals, by continually removing the perspiration that would otherwise be lodged about the feathers; and may perhaps act at the same time as a kind of stimulant to the skin. Although there are seldom above two or three on a small bird, yet from their size they must remove a great deal of what is excreted by the skin, as they appear seldom

to leave the bird they live upon, and even remain after the bird is dead. How far does the complete perfection displayed even in the simplest and meanest parts of creation baffle our comprehension, both with respect to their mechanism and utility, and evidence the hand that formed them!

M. W. Carolan.

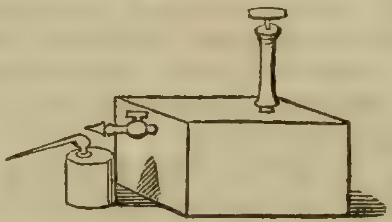
Article VII.

Description of a new Blow-Pipe. By H. I. Brooke, Esq.

(To Dr. Thomson.)

DEAR SIR,

I send you enclosed a drawing of a blow-pipe which I have lately had made upon a principle I believe entirely new in its application to this instrument, and which I have reason to believe, from the power which it possesses, and the facility with which it may be used, is an improvement upon any that has preceded it. The occasion of my having it made was to relieve the great inconvenience I felt in using



the common blow-pipe with the mouth. The first idea that suggested itself to me was to produce the jet of air from a sort of artificial mouth, or *moveable* receiver, of rather large dimensions, the capacity of which should be capable of gradual reduction by means of a spring; but it immediately occurred to me that the elasticity of the air itself, if forced into a *fixed* receiver, would be more uniform in its action than any spring, and might be regulated so as to produce a continued and more uniform jet. I accordingly applied to Mr. Newman, in Lisle-street, to make one upon this principle, to consist of a copper or iron vessel, into which the air is forced by a small condensing syringe, and from which it is suffered to escape through a tube of very small aperture, regulated by a stop-cock; and I have found it capable of affording a very intense and regular degree of heat. The form given in the drawing has been adopted by Mr. Newman for the convenience of packing into a small case; and he has also added to the syringe a screw, by means of which the receiver may be filled with oxygen or any other gas, which renders it more extensive in its application to chemical purposes, and probably so as to supersede the use of the common gazometer.

I am, dear Sir, your obedient servant,

Keppel-street, April 8, 1816. H. I. Brooke.

Article VIII.

An Account of a Trial of Dr. Reid Clanny's Lamp in some of the Newcastle Coal-Mines. By W. Reid Clanny, M.D.

(Read before the Royal Society, Dec. 7, 1815.)

On May 20, 1813, a paper of mine, on the means of procuring a steady light in coal-mines without the danger of an explosion, was honoured by a reading before the Royal Society, and accordingly I consider myself as in some measure pledged to lay before that Learned Body the following particulars, in continuation of the subject.

Since the above-mentioned time, explosions in this district have become more frequent, as may be readily understood by comparing the subjoined list with that of my former statement, inserted in the Philosophical Transactions for 1813.

By the falling of a stone from the roof of the mine, an explosion took place in the Hall Pit, at Fatfield, Sept. 28, 1813; by which 32 persons were killed, and four wounded. Three other explosions occurred in that mine at different times, by which three men were killed.

Upon Dec. 24, 1813, another explosion occurred at Felling; by which 23 persons were killed, and 21 much wounded.

The Hebburn Colliery exploded upon Aug. 12, 1814; and 11 men were killed, leaving nine widows and 27 children in the greatest distress.

An explosion at Leefield Colliery upon Sept. 9, 1814, killed four men, who left two widows and 12 children.

June 2, 1815, the Success Pit exploded; by which 54 were killed upon the spot, two were suffocated, and 15 very severely wounded, some of whom afterwards died.

The Tyne Main Colliery exploded upon June 5, 1815; by which one man was most severely scorched.

July 27, 1815, the Sheriff Hill Pit exploded; by which 10 men and boys were killed.

By some persons concerned in coal-mines it has been asserted that by ventilation, properly conducted, explosions may be avoided. I grant that by *proper* ventilation several calamitous accidents might in all probability have been prevented; but it may be fairly asked where those coal-mines are which may be said to be properly ventilated? As a case in point, the catastrophes at Felling Colliery may be instanced. In 1812 this colliery was understood to be one of the best ventilated and best regulated coal-mines in this district; yet on May 25, in that year, 92 persons were killed by an explosion; and 19 months afterwards, as stated above, though incredible pains and expense had been bestowed upon it by the humane

proprietors, an explosion occurred, by which 23 persons were killed, and 21 severely wounded.

Many of the old coal-mines are upon such a bad plan of ventilation, that the talents of the first viewers and engineers are insufficient to keep them working, whilst the poor pitmen are harassed with continual fears of destruction.

In this district there are several coal-mines that have only one shaft, which serves the double purpose of ventilation and working.

In a considerable number of collieries there are immense collections of carbureted hydrogen gas, which have been accumulating in several instances for many years, without the smallest chance of its ever being carried out of the mine. Under these and similar circumstances, need we be surprised at the increased frequency of explosions?

In my former paper some of the principal causes of explosions were described; and it may perhaps be acceptable to mention some others, with which I have lately become acquainted.

1. The great extent of collieries, and the incertitude which often prevails as to their boundaries, from the neglect of those immediately concerned, in not making correct maps and accurate records.

2. When the plan of the colliery is lost, or abstracted.

3. When sudden eruptions of carbureted hydrogen gas unexpectedly mix the whole circulating mass of air up to the firing point.

4. When the barometer stands below 29°, and the wind is at S. E., the atmospheric current becomes too light to sweep off the increased discharge of inflammable air which then issues out of every part of some mines.

5. When the inflammable air prevails between the workmen and the upcast shaft, and a fall of stone from the roof, or other causes, occur to force back the atmospheric current towards the downcast shaft.

About four years ago I constructed a small lamp of a strong glass, the bottom of which was shut, with the exception of a small opening to admit the tube from the bellows, for throwing in the necessary quantity of air to support the combustion of the candle within the lamp.

I found that it safely insulated the candle; but I was soon told that it would never answer for the purpose intended; that frequently large pieces of stone fell from the roof, which would destroy the lamp; and as the candle would thereby come in contact with the mass of inflammable air of the mine, an explosion would occur as a matter of course. I also found that valves would not suit; for the expansive force of the explosions within the lamp threw open the valves, and allowed a communication to take place between the candle and the surrounding atmosphere; besides, the water, when used as a valve, not only keeps the apparatus cool, but ensures

perfect safety. I was also told that frequently large masses of coal are struck off from the sides of the mine, whilst the pitmen are hewing out the coal, as I have witnessed myself; and should a piece of coal strike the lamp upon the side, it would of course break it, and expose the inflammable gas to an instantaneous explosion.

The lamps, as they are now constructed, have the following qualities. The piece of glass in front is so strong as to be ensured to carry a ton weight. The rest of the lamp is copper, or strong block tin, supported by three strong iron pillars.

The lamp is now constructed of so small a form, that it may be put into a great coat pocket. A copper lamp may be made for about 30*s.* or 35*s.*; and if it be of block tin, the expenses will not exceed half the latter sum.

By means of a very simple apparatus attached to the lamp, I can light the candle with a common match of hyper-oxymuriate of potash and concentrated sulphuric acid in an atmosphere of inflammable air.

And should the lamp be upset (which can only be done wilfully), the candle is instantly extinguished.

I can manage the bellows in such a way that the candle continues to burn with the fire-damp of most mines; and the syphon, which may be understood by the engraving of my lamp in the Transactions of the Royal Society for 1813, is for the purpose of keeping the water upon a level within and without the lamp, while the candle continues to burn, though the inflammable air be at the firing point.

By a small piece of machinery, which costs about 20*s.*, the bellows may be urged so as to give a constant and sufficient supply of air for the candle for two hours, without winding up; but if the proportion of inflammable air be very great, a slight explosion generally takes place within the lamp, which extinguishes the candle.

When this latter circumstance occurs, the lamp is to be re-lighted, and air given to the candle in the following manner. To the valve of the bellows a leathern nose, or tube, is attached, which, being of such a length as to reach to that part of the mine where there is a current of good atmospheric air, by this means the lamp will of course continue to afford a clear light in the midst of inflammable air.

It sometimes happens that there is a deficiency of oxygen in the atmosphere of a coal-mine. In those cases a very small taper may be used in the lamp, the combustion of which may be supplied by a goat-skin full of atmospheric air, or a few bladders of oxygen gas.

The enormous expense of steel-mills in some mines almost exceed belief. I am informed that in one working of a colliery in this neighbourhood, the expense of steel mills is about 30*l.* every fortnight; so many of them have to be kept at work at a time to give any thing like a sufficient light.

When I say that the expense of one of my lamps, if made of block tin, does not exceed 17*s*., it is needless to remark that upon the score of expense there is no comparison.

I made many fruitless efforts to descend a mine charged with inflammable air. At one time the person who invited me to his house, at a considerable distance from Sunderland, went from home when I arrived. Two years afterwards, when I arrived at a person's house (likewise at a considerable distance), who had promised to descend a colliery with me, I found that he had just examined all the parts of the mine, as he said, and that there was no inflammable air to be found in any part of it. This I afterwards found was not the case.

Indeed, the ungenerous opposition I have met with is almost incredible; but the train of miseries detailed in this and my former paper leaves no room for delicacy, and the state of the case demands that some remedy should be applied.

In the mean time all the men of science who came into this neighbourhood examined the lamp, and gave it their entire approval.

Vexed at such treatment, I wished to forget the subject, and let things run their course, when, immediately after an explosion in one of the mines, a sensible and humane letter appeared in the Morning Chronicle newspaper, signed J. H. H. Holmes, which, after much close reasoning upon the subject, put the question whether any of my lamps had been used in those coal-mines which had recently exploded?

After a time, and as no person appeared to take up the subject, I thought it my duty to state, amongst other things, that no person had ever used my lamp in a coal-mine.

From this public correspondence a private one arose; and not long afterwards this Gentleman did me the honour to visit me, and immediately commenced an investigation of the coal-mines, in order to give some general information upon this very interesting subject.

It will be unnecessary, after the preceding statements, to trouble the Royal Society with any further particulars, except the two following certificates, which were drawn up, and signed, according to their respective dates, on the spot where the trials were made, which, it is expected, place the value and security of the lamp beyond a doubt. The trial within the mine was conducted at the place where 24 persons were not long since killed by an explosion.

First Certificate.

(Copy.)

Herrington Mill Pit, Oct. 16, 1815.

An experiment took place this day on Dr. Clanny's lamp for preventing explosions in coal-mines. It was effected at the mouth of

the upcast shaft of the Herrington Mill Pit, by means of inflammable air obtained from a cast-iron tube communicating with the Hutton seam, and witnessed by the undersigned.

In order to ascertain the quality of the gas given out at this tube, a bladder was filled from it, and on trial its contents proved to be carbureted hydrogen gas of the purest nature.

One end of a leaden pipe was affixed to the iron tube; the other end placed within a room which was quite closed up, except at the door where the pipe entered. In a very short time the carbureted hydrogen gas became mixed with the atmospheric air of the room up to the firing point, when the lamp, with a lighted candle within it, was carried into the centre of the room; and after conveying two or three draughts of air through the bellows, an explosion took place, which extinguished the candle without communicating to the surrounding inflammable atmosphere. This experiment was practised a second time, and the same results followed.

On witnessing the experiment, the under-mentioned Wm. Patterson and Joseph Gleghorn declared that they would go into any part of a mine without any fear, if lighted by this lamp.

(Signed) J. H. H. Holmes.
Wm. Patterson.
Jos. Gleghorn.
Anth. Hopper.
George Patterson.

SECOND CERTIFICATE.

(Copy.)

Monday, Nov. 20, 1815.

Dr. Clanny and Mr. Holmes (one of the undersigned) left Sunderland this day, for the purpose of experimenting upon Dr. C.'s lamp, in some of the most inflammable parts of a coal-mine; for notwithstanding that it was satisfactorily experimented upon on Oct. 16, within a room filled with inflammable air at the firing point, it was thought expedient to carry it into those parts of a mine where its benefit must ultimately be produced.

They descended the Herrington Mill Pitt, which is 101 fathoms in depth from the surface; and having proceeded upon the examination of the mine, found the most inflammable part at the bottom of a staple, which was closed about 20 feet down by scaffolding, and made to communicate with the Hutton seam, which, being now worked out, is full of inflammable air. [And from this the tube runs by which we were enabled to make the experiments of Oct. 16, 1815.—W. R. C.]

Much caution was required in keeping the candles from approaching too near the staple, as their appearance, when held near the mouth, clearly indicated that, had they been introduced too far, an explosion must necessarily have followed.

Dr. Clanny and John Birkbeck, a man employed in the mine, stood at the top. Mr. Wm. Patterson, a very able and intelligent man, descended half down the staple, and Mr. Holmes stood upon the scaffolding. The lamp with the lighted candle was handed by Dr. Clanny to Mr. Patterson, who descended with it to Mr. Holmes; and after the bellows of the lamp were urged a few seconds, a slight flash occurred within the body of the lamp, and the candle was immediately afterwards extinguished. No particular caution was observed with the lamp, as a confidence in its security resulted from the experiments of Oct. 16; and if a further proof of this was necessary, it was afforded by the presence of Mr. Patterson and Birkbeck, both of whom declared "that if the candle had communicated with the circumambient air on the spot where the experiment took place, the mine would have been blown to pieces."

(Signed) J. H. H. Holmes.
Wm. Patterson.
Jno. Birkbeck.

Article IX.

Analyses of Books.

Nova Genera et Species Plantarum quas in Peregrinatione ad Plagam Æquinoctialem Orbis Novi collegerunt, descripserunt partim adumbraverunt Amat. Bonpland et Alex. de Humboldt. Ex Schedis autographis Amati Bonplandi in Ordinem digessit Carol. Sigismund. Kunth. Accedunt Tabulæ Æri incisæ, et Alexandri de Humboldt Notationes ad Geographiam Plantarum spectantes. Tomus Primus. Paris, 1815. super-royal 4to.

(With a Plate.)

Humboldt, in a long introduction to this volume, gives an account of the different publications in which he has consigned the immense number of facts which he collected during his residence in South America, and informs the reader that the object of the present work is to put botanists, at a small expense, in possession of the descriptions of the American plants which he and his fellow traveller collected. We doubt, however, whether a book printed like this, on the largest sized quarto paper, and with a wide margin, can be afforded at a small expense. Nor can there be any doubt that Humboldt would have been much more useful to science if he had published all his works in a less expensive form, and had been somewhat more concise in his style. Fortunately the desire for magnificent books is losing ground in this country, and we wish to

see a simplification in the taste of bibliography all over the world. The magnificence and expense of Humboldt's books must have rendered their sale exceedingly limited; a circumstance which could not but tend very materially to circumscribe his reputation, and to prevent the general diffusion of the facts which he published; for the purchasers of very expensive books are very seldom those who are best qualified to appreciate their merit, or to draw advantage from them. We are persuaded that Baron von Humboldt would very materially increase his reputation, and at the same time confer an important obligation on science, if he would republish all his works on South America in a small octavo form, and freed as much as possible from all repetitions and unnecessary details.

Humboldt and Bonpland, in their five years' travels in South America, from north latitude 12° to south latitude 23°, collected 5,800 species of plants. Of these 5,500 were phanerogamous plants, 3,000 of which were new, and unknown before to botanists. That this number is very considerable, will be evident from this, that in the Systema Vegetabilium of Willdenow, published from 1797 to 1811, the whole phanerogamous plants of South America do not exceed 3,188 species; while the plants of New Holland at present known do not exceed 3,800 species.

The species of phanerogamous plants known to grow in South America within the Tropics, including those that have been lately added to the list by Humboldt and Bonpland, Ruiz and Pavo, Persoon, Mutis, &c. amount to 13,000.

Botanists at present are acquainted altogether with 44,000 species of plants; while the whole number mentioned by the Greeks, Romans, and Arabians, does not exceed 1,400. The proportion of plants which grow in latitudes 0°, 45°, and 68°, are as the numbers 12, 4, 1. The mean annual temperature in these regions is $81\frac{1}{2}°$, $55\frac{1}{2}°$, $32\frac{1}{3}°$; the mean summer temperature is $82\frac{1}{2}°$, 70°, $53\frac{1}{2}°$.

Within the tropics the monocotyledinous plants are to the dycotyledinous as one to six; between latitudes 36° and 52°, as one to four; and at the polar circle, as one to two. In Germany, the monocotyledinous plants are to the whole phanerogamous plants as 1 to $4\frac{1}{2}$; in France, as 1 to $4\frac{2}{5}$. The same proportion holds in North America; and likewise, according to Mr. Brown, in the temperate zone of New Holland; while in Iceland and Lapland the monocotyledinous plants are to the whole phanerogamous as one to three.

The annual monocotyledinous and dicotyledinous plants in the temperate zone constitute $\frac{1}{6}$th part of the whole phanerogamous plants. In the torrid zone they scarcely amount to $\frac{1}{20}$th part; and in Lapland to $\frac{1}{30}$th.

The following table exhibits the number of species of the natural families of phanerogamous plants which grow spontaneously in France, Germany, and Lapland, with the ratio of each to the whole number of phanerogamous plants growing in the country.

Natural Families.	Number of Species in			Ratio of each Family to the whole in		
	France.	Germany.	Lapland.	France.	Germany.	Lapland.
Cyperoideæ	134	102	55	$\frac{1}{27}$	$\frac{1}{18}$	$\frac{1}{9}$
Gramineæ	284	143	49	$\frac{1}{13}$	$\frac{1}{13}$	$\frac{1}{10}$
Junceæ	42	20	20	$\frac{1}{86}$	$\frac{1}{94}$	$\frac{1}{25}$
Three preceding families ..	460	265	124	$\frac{1}{8}$	$\frac{1}{7}$	$\frac{1}{4}$
Orchideæ	54	44	11	$\frac{1}{67}$	$\frac{1}{48}$	$\frac{1}{45}$
Labiatæ	149	72	7	$\frac{1}{24}$	$\frac{1}{26}$	$\frac{1}{71}$
Rhinantheæ et Scrofulariæ.	147	76	17	$\frac{1}{24}$	$\frac{1}{24}$	$\frac{1}{29}$
Boragineæ	49	26	6	$\frac{1}{7}$	$\frac{1}{72}$	$\frac{1}{83}$
Ericeæ et Rhododendra ..	29	21	20	$\frac{1}{125}$	$\frac{1}{90}$	$\frac{1}{25}$
Compositæ	490	233	38	$\frac{1}{7}$	$\frac{1}{8}$	$\frac{1}{13}$
Umbelliferæ	170	86	9	$\frac{1}{34}$	$\frac{1}{22}$	$\frac{1}{55}$
Cruciferæ	190	106	22	$\frac{1}{19}$	$\frac{1}{18}$	$\frac{1}{23}$
Malvaceæ	25	8	0	$\frac{1}{145}$	$\frac{1}{235}$	0
Caryophylleæ	165	71	29	$\frac{1}{22}$	$\frac{1}{27}$	$\frac{1}{17}$
Leguminosæ	230	96	14	$\frac{1}{16}$	$\frac{1}{18}$	$\frac{1}{35}$
Euphorbiæ	51	18	1	$\frac{1}{71}$	$\frac{1}{104}$	$\frac{1}{497}$
Amentaceæ	69	48	23	$\frac{1}{52}$	$\frac{1}{39}$	$\frac{1}{21}$
Coniferæ	19	7	3	$\frac{1}{192}$	$\frac{1}{269}$	$\frac{1}{165}$
Phanerogamous	3645	1884	497	0	0	0

France lies between latitude $42\frac{1}{2}°$ and 51°. The mean temperature of the year is 62°—52°. Mean temperature of the summer 75°—66°. The months in which the heat exceeds 51 are March—November, May—September.

Germany lies between latitude 46° and 54°. The mean temperature of the year is $54°—47\frac{1}{3}°$. The mean of summer is 70°—64°. The months in which the temperature exceeds 51° are April—October, May—September.

Lapland lies between latitudes 64° and 71°. The mean temperature of the year is from 34°—17°. The mean temperature of summer is 55°—45°. The months in which the temperature exceeds 51° are June—August, June—July.

The following is the distribution of the natural families in the temperate zone of North America, according to the Flora of Pursh:—

	Species.		
Cyperoideæ	71	or	$\frac{1}{40}$
Gramineæ	275	....	$\frac{1}{10}$
Junceæ	19	....	$\frac{1}{152}$
Three preceding families	365	....	$\frac{1}{8}$
Labiatæ	78	....	$\frac{1}{40}$
Rhinantheæ and Scrofulariæ	79	....	$\frac{1}{36}$
Ericeæ and Rhododendra	80	....	$\frac{1}{36}$
Compositæ	454	....	$\frac{1}{6}$
Umbelliferæ	50	....	$\frac{1}{57}$
Cruciferæ	46	....	$\frac{1}{62}$
Malvaceæ	23	....	$\frac{1}{125}$
Caryophylleæ	40	....	$\frac{1}{72}$
Leguminosæ	148	....	$\frac{1}{19}$
Amentaceæ	113	....	$\frac{1}{25}$
Coniferæ	28	....	$\frac{1}{103}$
Total phanerogamous plants	2890		

The following is the proportion of the different families of plants observed by Humboldt and Bonpland in South America within the tropicks :—

	Species.		
Cyperoideæ	68	or	$\frac{1}{57}$
Gramineæ	256	....	$\frac{1}{15}$
Junceæ	9	....	$\frac{1}{430}$
Three preceding families	333	....	$\frac{1}{11}$
Labiatæ	95	....	$\frac{1}{40}$
Ericeæ and Rhododendra	30	....	$\frac{1}{129}$
Compositæ	600	....	$\frac{1}{6}$
Umbelliferæ	30	....	$\frac{1}{129}$
Cruciferæ	19	....	$\frac{1}{204}$
Malvaceæ	80	....	$\frac{1}{47}$
Leguminosæ	314	....	$\frac{1}{12}$
Phanerogamous	3880		

The number of genera bear a greater proportion to that of the species in cold and hilly countries than in warm and level ones.

The following table exhibits the ratio of each family to the whole phanerogamous plants in each zone :—

	Torrid Zone. Mean Temp. $80\frac{1}{2}°$.	Temperate Zone. Mean Temp. 50°—57°	Frigid Zone. Mean Temp. 32°—30°	
Agamæ cellulosæ..	1 : 5	1 : 2	1 : 1	
Filices		1 : 60	1 : 25	Ger. $\frac{1}{48}$. France $\frac{1}{73}$.
Monocotyledones .	1 : 6	1 : 4	1 : 3	
Cyperoideæ	1 : 60	1 : 30	1 : 9	
Gramineæ	1 : 15	1 : 12	1 : 10	
Junceæ	1 : 400	1 : 90	1 : 25	N. Am. $\frac{1}{150}$. Fr. $\frac{1}{80}$.
Three preceding families	1 : 11	1 : 8	1 : 4	
Labiatæ	1 : 40	1 : 25	1 : 70	N. Am. $\frac{1}{40}$. Fr. $\frac{1}{24}$.
Ericineæ and Rhododendra .	1 : 130	1 : 100	1 : 25	N. Am. $\frac{1}{36}$. Fr. $\frac{1}{125}$.
Compositæ	1 : 6	1 : 8	1 : 13	
Rubiaceæ	1 : 29	1 : 60	1 : 80	Fr. $\frac{1}{73}$. Ger. $\frac{1}{70}$.
Umbelliferæ	1 : 2000	1 : 30	1 : 60	N. Am. $\frac{1}{57}$. Fr. $\frac{1}{34}$.
Cruciferæ	1 : 3000	1 : 18	1 : 24	N. Am. $\frac{1}{62}$. Fr. $\frac{1}{19}$.
Malvaceæ........	1 : 50	1 : 200	0	N. Am. $\frac{1}{125}$. Fr. $\frac{1}{145}$. Ger. $\frac{1}{235}$.
Leguminosæ	1 : 12	1 : 18	1 : 35	
Euphorbiaceæ	1 : 35	1 : 80	1 : 500	
Amentaceæ exclusis casuarin.		1 : 45	1 : 20	

The number of lofty trees in North America is much greater than in Europe. In North America there are 137 species of trees, whose trunks exceed the height of 30 feet, while in Europe there are scarcely 45 species.

The plants of the torrid zone, as had been already observed by Mr. Brown, extend further through the southern temperate zone than through the northern. This is to be ascribed to the greater influence of the ocean in the southern hemisphere, in moderating the rigour of winter.

No firs are to be found on the mountains of South America between the tropics, though they are very abundant in North America.

In the temperate zones we frequently find the same species of plants growing together in clusters, as is the case with erica vulgaris, polygonum aviculare, pinus sylvestris, &c.; but such associations of plants very seldom occur in the torrid zone, where the woods consist of a great variety of trees nearly equably mixed.

Several species of plants, though not many in number, are common to the north temperate zones both of the old and new con-

tinent: and the same observation applies to the south temperate zones. A few musci, filices, and lichens, and monocotyledinous plants are common to the American torrid zone and to Europe; but not a single bicotyledinous plant, as far as has been hitherto observed. Hence it would seem, that the distribution of the monocotyledinous and bicotyledinous plants is not subjected to the same law.

In order to form a conception of the distribution of the different plants in each continent, it is necessary to know how the temperatures vary in each. For this purpose M. Humboldt has constructed the following table:—

Georgia, Missisippi, Lower Egypt, Madeira.

	Lat.		Mean Temp.
Natches	31°	28′	18·2° Cent. Ther.
Funchal	32	37	20·4
Orotava	28	25	21·0
Rome	41	53	15·8
Algiers	36	48	21·1
Difference	7	0	2·3

Virginia, Kentucky, Spain, South of Greece.

Williamsburgh	38°	0′	14·5°
Bourdeaux	44	50	13·6
Montpelier	43	36	15·2
Rome	42	53	15·8
Algiers	36	48	21·1
Difference	7	0	4·3

Pensylvania, Jersey, Connecticut, Latium, Rumelia.

Philadelphia	39°	56′	12·7°
New York	40	40	12·1
St. Maloes	48	39	12·5
Nantes	47	13	12·6
Naples	40	50	17—18
Difference	7	0	5·3

Ipswich	42°	38′	10·0°
Cambridge	42	25	10·2
Vienna	48	12	10·3
Manheim	49	29	10·7
Toulon	43	7	16·7
Rome	41	53	15·3
Difference	6	30	6

Lower Canada, Nova Scotia, Middle of France, South of Germany.

Quebec	46°	47′	5·5°	Cent. Ther.
Upsala	59	51	5·5	
Padua	45	24	13·7	
Paris	48	50	10·8	
Difference	13	0	7	

Labrador, South of Sweden, Courland.

Nain	57°	0′	−3·0°
Okak	57	20	−1·1
Umeo	63	50	+0·7
Enontekis	68	30	−2·8
Edinburgh	55	57	+8·8
Stockholm	59	20	+5·7
Difference	11	0	9·5

The differences in this table were determined by the method of interpolation. Humboldt has concluded from it, that the difference between the mean temperature of the same latitudes in America and the old Continent is as follows: expressed in degrees of the centigrade thermometer.

Latitude.	Mean Temperature.		Difference.
	Old Continent.	New Continent.	
0°	27·5°	27·5°	0°
20	25·4	25·4	0
30	21·4	19·4	2
40	17·3	12·5	4·8
50	10·3	3·3	7
60	4·8	− 4·6	9·4

If the mean temperature at the equator be reckoned 1, we shall find the half of that temperature in the old Continent at latitude 45°; in the new Continent at latitude 39°. The difference between the mean temperatures in the two Continents will be better seen from the following table :—

	Old Continent.	New Continent.
Lat. 0° Mean Temperature	1·00	1·00
30	0·77	0·70
40	0·63	0·45
45	0·48	0·30
50	0·37	0·12

Between latitude 38° and 50° in the old Continent, the mean temperature differs 12°; in the new Continent it differs 16·5°: so that in Europe a degree of latitude occasions a difference of 0·63°; in America of 0·87°. In both Continents the zone in which the temperature decreases most rapidly is contained between the parallels 40° and 45°.

Zones.	Diminution of heat corresponding to 10° in latitude. Old Continent.	New Continent.
0°— 20°	2·1°	2°
20 — 30	4	6
30 — 40	4·1	7
40 — 50	7	9
50 — 60	5·5	7·4
0 — 60	22·7	31·4

Nearly the same difference exists between the mean temperature of the eastern and western parts of the old Continent, as between the western parts of the old Continent and America, as will appear from the following table:—

	Lat.		Mean Temp.
St. Maloes	48°	39′	12·5°
Amsterdam	52	22	11·9
Copenhagen	55	41	7·6
Upsala	59	21	5·5
Naples	40	50	17·5
Vienna	48	11	10·3
Warsaw	52	14	9·2
Moscow	55	45	4·5
Petersburgh	59	56	3·8
Pekin	39	54	12·7

But though the mean temperature of the year in the middle latitudes of North America be the same as it is in Europe seven degrees further north, the temperatures of the different seasons by no means agree in these circles of equal temperature. The winters are colder and the summers warmer in North America than in Europe. At Philadelphia the summer is as hot as at Rome and Montpelier; while the winter corresponds with that at Vienna. At Quebec the summer is warmer than at Paris, the winter colder than at St. Petersburgh; but in the north of China there is a greater difference between the heat and cold than in North America.

Places.	Lat. N.	Mean ann. temp.	Mean Temperature. Winter	Spring	Summ.	Autumn	Coldest month.	Hottest month.	Differ. in the heat of these months.
Philadel.	39° 56′	12·7°	+1·1°	11·7°	24·0°	13·4°	+ 0·4°	25.0°	24·6°
Pekin ..	39 54	12·7	−3·1	13·5	28·1	12·4	− 4·1	29 1	33·2
Nantes ..	47 13	12·6	+4·6	12·5	20·4	13·1	+ 3·9	21·4	17·5
Rome ..	41 53	15·8	+7·7	14·3	24·0	17·1	+ 5 6	25·0	19·4
Paris....	48 50	10·8	+3·4	9·8	18·8	10·7	+ 2·2	19·7	17·5
Quebec ..	46 47	5·4	−9·9	3·8	20·0	7·8	−10·7	23·0	33·7
Upsala ..	59 51	5·5	−3·9	4·1	15·7	6·0	− 4·3	16·6	20·9

In North America, as far as latitude 48°, the summers are four centigrade degrees hotter than in the corresponding latitude in Europe; hence the reason why magnolias and other equinoxial plants appear so far north in America. In treating of the geographical distribution of plants, it is of consequence to distinguish between the mean annual temperature and the mean temperature of summer.

Mean annual temp. in each Continent.	Latitudes. A. America. E. Europe.	Mean heat of summer.	Diff. between the temp. of the equator and Mean annual temp.	Temp. of summer.	Ratio of the mean annual summer temp.
15° (Rome, 15·8°.)	A 36° E 43	26·7° 23·0	12·5° 12·5	0·8° 4·5°	1 : 1·7 1 : 1·5
10° (Paris, 10·8°.)	A 42½ E 49½	21·8 18·0	17·5 17·5	5·7 9·5	1 : 2·1 1 : 1·8
5° (Stockholm, 5·7°)	A 48 E 60	19·5 15·1	22·5 22·5	8 12·4	1 : 3·9 1 : 3
0° (North Lapl.)	A 54 E 68½	12·0 11·5	27·5 27·5	15·5 15	1 : 12 1 : 11·5

From Barton's observations it appears, that America is warmer to the west of the Aleghany mountains than to the east of them; hence certain plants are found four degrees further north in Louisiana and on the Ohio, than on the borders of the Atlantic. Humboldt supposes that this difference does not extend further north than latitude 42°. Beyond Lake Superior and at Hudson's Bay it is said that the earth is perpetually frozen at the depth of three feet from the surface, which prevents the inhabitants from digging wells. The same thing happens in Siberia in the latitude of 62° on the banks of the river Lena; while in Lapland at the tempe-

rature of 70° near Wadsoe, the temperature of the earth is 2·2° above the point of congelation.

Between the tropics America is as hot as in the old Continent, as may be seen in the following table:—

Old Continent.	Mean Temp.	*New Continent.*	Mean Temp.
Senegambia	26·5°	Cumana	27·7°
Madras	26·9	Antilles	27·5
Batavia	25·2	Vera Cruz	25·6
Manilla	25·6	Havannah	25·6

On the south side of the line the temperatures appear less than on the north; thus Rio Janeiro and Havannah are nearly at the same distance from the equator. The following is the mean temperature of similar months in each:—

Rio Janeiro.		*Havannah.*	
June	20·0°	December	22·1°
July	21·2	January	21·2
January	26·2	July	28·5
February	27·0	August	28·8

On the coast of Peru the temperature is considerably diminished by the perpetual cloudiness of the sky, and by a strong sea current setting in from Cape Horn. The mean daily temperature is scarcely 20° — 22·5°, and that of the night 15° — 17°. Humboldt has seen the thermometer on the sea shore south latitude 12° 2′, as low as 13°. From the tropic to latitude 34° south, the mean temperature of the southern hemisphere scarcely differs from that of the northern. The mean temperature at Port Jackson (lat. 33° 51′) is 19·3°; at the Cape of Good Hope (lat. 33° 55′) 19·4°; at Buenos Ayres (lat. 34° 36′) 19·7°. In the northern hemisphere latitude 34° corresponds to a mean temperature of 19·8°. Between latitudes 34° and 57°, there is a greater difference between the temperatures of summer than of winter. The winters in the south are not colder, but the summers are considerably more so than in the northern hemisphere. On the coast of Patagonia (lat. 48°—58°) the mean heat of summer does not exceed 6°—8·2°; while at St. Petersburgh and Umea it rises to 18·7° and 17° (lat 59° 56′, and 63° 50′). At Chorruca in the Straits of Magellan (lat. 53°—54°) it snows almost every day during the summer, and the thermometer in December did not rise higher than 11·2°; whereas Von Buch observed it at latitude 70° in Lapland as high as 26·7. Cook and Forster did not observe the thermometer rise in south latitude 60° higher than 2·2°. In Lapland in the latitude of 70°, the fir grows to the height of 60 feet. Whereas at the Straits of Magellan, and in Staaten's Land, much nearer the equator, scarcely any trees can

grow. The mean temperatures in the two hemispheres do not differ so much from each other as the summer temperatures. In south latitude 48° the summer temperature is equal to the winter temperature of Toulon, Cadiz, and Rome.

Mountains 1000 fathoms in height at latitude 46°, enjoy the mean temperature of Lapland, while the same heights between the tropics enjoy the temperature of Sicily and Calabria: for this height occasions a diminution of temperature amounting to 12°. Hence 500 fathoms correspond with 9° 30′ of latitude; so that 50 fathoms is very nearly equivalent to half a degree of latitude.

We regret that we cannot insert here Humboldt's catalogue of the distribution of plants within the tropics of South America, according to the height of the different places above the level of the sea; but the following table exhibits the mean temperature at different heights between the tropics in the new Continent.

Height in fathoms.	South America, between lat. 0° and 10°, S. and N.			Mexico, between lat. 17°—21°, N.		
	Mean ann. temp.	Var. in the temp. of the whole year.		Mean ann. temp.	Var. in the temp. of the whole year.	
0	27·5°	11·5°	Cumana	26°	16°	Vera Cruz
500	20·5	12·7	Caracas	19·8		Encero
1000	18·0		Popayan	18	22	Valladolid
1500	13·5	16	Quito	14		Real del Monte
2000	6·8			7·5		
2500	1·5	19	Pichinca	—1		

The higher we ascend above the level of the sea, and the further we advance from the equator, the greater is the difference between the temperature of the different seasons of the year. The following table exhibits the difference between the mean temperature of the two hottest and coldest months in different latitudes.

	Lat.		
Cumana	10°	27′	2·4°
Vera Cruz	19	11	5·6
Havannah	23	8	7·4
Natchez	31	28	17·4
Philadelphia	39	56	24·6
Quebec	46	47	33·0
Nain	57	0	35·3

The temperature of elevated situations in Europe is not so well known as in South America, where there are cities at a greater height than the top of the Pyrenees, and houses more elevated than the summit of the Peak of Teneriffe. The following table contains the facts which Humboldt has been able to collect on the subject:—

North Latitude, 45°—47°.

	Height in fathoms.	Ann. mean temp.	Mean of coldest month.	Mean of hottest month.
Sea shore	0	12·5°	+2·4°	21·0°
Geneva	180	9·6	+1·2	19·2
Tegernsee	382	5·8	−5·5	15·2
Peissenberg	511	5	−6·2	13·9
Chamouix	528	4		13·0
Monastery of St. Gothard	1065	−0·9	−9·4	7·9
Col de Geant	1763	−6		2·5

In the temperate zone of our Continent, when the mean heat of the month amounts to

5·5°	the amygdalus persica blossoms	
8·2	prunus domestica	
11·0	betula alba.	

At Rome the mean temperature is 11° at the end of March; at Philadelphia, in April; at Paris, in May; at Upsala, in the middle of June. At the monastery of St. Gothard the betula alba cannot blossom, because the mean heat of the hottest month scarcely amounts to 9°. Between north latitude 40° and 60°, the hottest month adds about 9° to the mean temperature of the year; and beyond 60° the addition is greater.

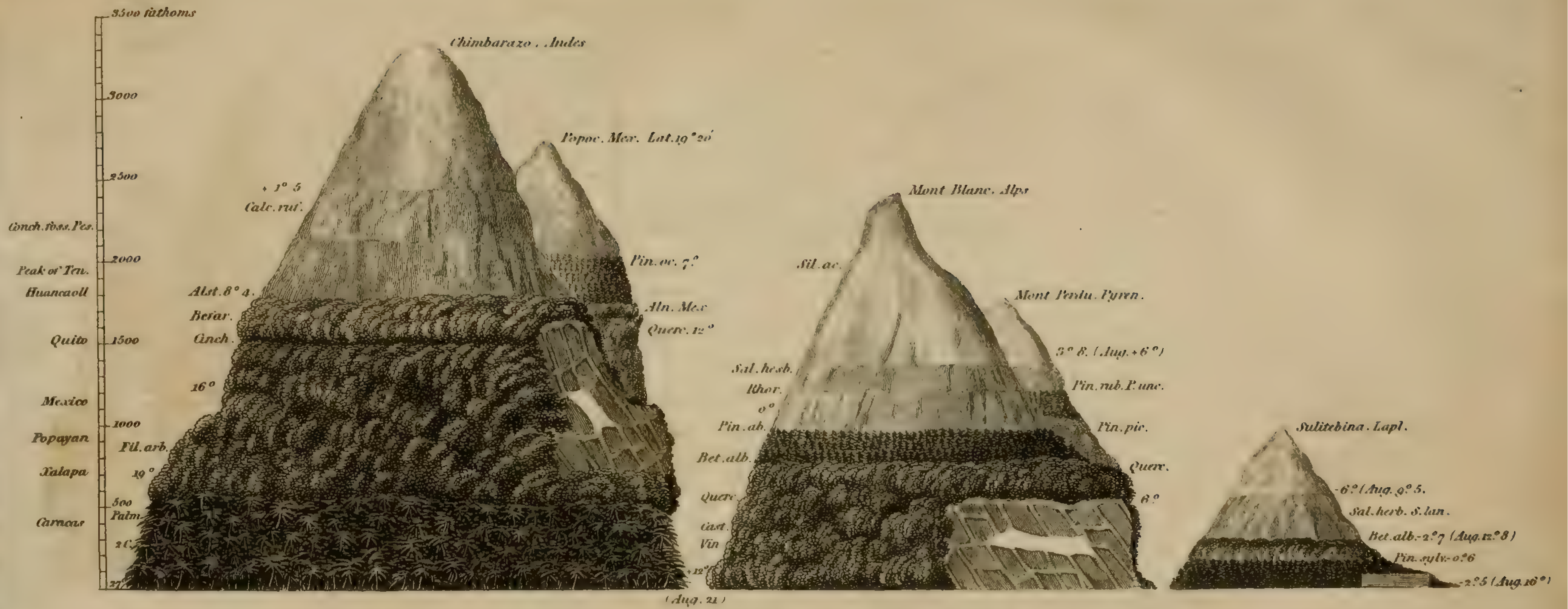
In mount Caucasus, between latitude 42° and 43°, the lower limit of perpetual snow is at the height of 1650 fathoms. In the Pyrenees, between latitude 42½° and 43° the lower limit of perpetual snow is at the height of 1400 fathoms. In the Alps, between latitude 45¾° and 56½°, it is at the height of 1370 fathoms. Humboldt gives the height to which different plants reach on these mountains, and we are sorry our limits will not allow us to insert them here.

In the temperate zone, as we advance northwards, the coldness of the winter increases at a much greater rate than the heat of the summer diminishes. Thus at Enonlekis, in latitude 68½°, the temperature of July is as hot as at Edinburgh; but between the tropics, the temperature on mountains at no season of the year equals that at the sea shore. Hence in the temperate zone we find the same plants frequently in low and elevated situations; but this is never the case between the tropics.

In Lapland, between latitude 67½° and 70°, the inferior limit of perpetual snow is at the height of 550 fathoms.

The reason why plants vegetate with so much greater rapidity in Lapland and Norway than further south, is because the increment of temperature is much greater, and because the temperature of the earth in winter is several degrees above that of the air. The following table exhibits the most remarkable circumstances respecting temperature in the three different zones.

English.



TORRID ZONE, Lat. 0° to 10°
(Humboldt. Bonpland)

TEMPERATE ZONE, Lat. 42° to 46° N.
(Wahlenberg. Buch. Ramond. Decandolle.

FRIGID ZONE. Lat. 68° N.
(Buch. Wahlenberg)

Dr Thomson's Annals for Baldwin, Cradock & Joy, Paternoster Row, May 1. 1816.

	Torrid Zone.		Temperate Zone.				Frigid Zone.
					Alps, lat. $45\frac{3}{4}°$ to 46.		
	Lat. 0°. Andes Quit.	Lat. 20°. Mount of Mexico.	Caucasus Lat. $42\frac{1}{2}°$	Pyrenees Lat. $42\frac{3}{4}°$	Aspect northerly	Aspect souther	Lat. $67\frac{1}{3}°$ —70°. Lapland.
Infer. limit of perpetual snow....	2460 fat.	2350 f.	1650 f.	1400 f.	1370 f.	1370 f.	550 f.
Mean annual heat at that height ..	$1\frac{1}{2}°$			$-3\frac{1}{2}°$	$-4°$		$-6°$ cen.
Mean heat of winter at that height	$1\frac{1}{3}°$				$-10°$		$-20\frac{1}{2}°$
Mean heat of August at that height	$1\frac{3}{4}°$				+6		$+9\frac{1}{2}°$
Distance between trees and snow .	660 f.	350 f.	650 f.	230 f.	450 f.	320 f.	300 f.
Upper limit of trees	1800 f.	2000 f.	1000 f.	1170 f.	920 f.	1050 f.	250 f.
Last species of trees towards the snow	Escallonia, alstonia.	Pinus occident.	Betula alba.	Pinus rubra. P. uncin.	Pinus abies.	Pinus larix.	Betula alba.
Upper limit of the ericineæ....	Befariæ (1600 f.)		Rhodod. caucas. (1380 f.)		Rhodod. ferrug. (1170 f.)		Rhod. laponicum. (480 f.)
Distance between the snow and corn	860 f.		630 f.		700 f.		450 f.

We shall terminate this very imperfect outline of Humboldt's curious *Prolegomena*, by exhibiting his plate (Plate XLIX.) of the distribution of plants at different heights in the torrid, temperate, and frigid zones.

The names of the plants are put at the height at which they cease to grow. The numbers indicate the annual temperature according to the centigrade scale: those between hooks the temperature of August. The fathom is 6 French feet = 6·39453 feet English.

Article X.

Proceedings of Philosophical Societies.

LINNÆAN SOCIETY.

On Tuesday, April 2, a paper by M. Decandolle was read, describing two new genera belonging to the family of rosaceæ, to which he gave the names of kerria and purshia. The first is a Chinese plant, which has been long common in our gardens, under the name of corchorus japonicus. The second is an American plant, first described by Mr. Pursh, as a species of Tigarca.

At the same meeting a paper was read, describing a new species of potatoe growing wild in New Granada, in South America, at a very great height above the level of the sea. The Spanish Ecclesiastic who found it proposes to call it solanum papa. The potatoes are white, and round, and well tasted. The fruit is oblong, which seems to constitute the chief difference between it and our potatoe.

GEOLOGICAL SOCIETY.

Feb. 16, 1816.—A report by Dr. Granville on a memoir by M. Methuen, entitled Découverte de la Manière dont se forment les Cristaux, was read.

This memoir was transmitted in manuscript to Dr. Granville by M. Gillet de Laumont, who in a subsequent letter expresses his great confidence in the veracity of M. Methuen, the author of this remarkable discovery.

The formation of capillary crystals of alum and of sulphate of iron on the decomposing surface of aluminous schist, in a situation where no moisture, except that dissolved in the air, had access, first directed M. M.'s attention to the subject. In order to destroy or confirm his hypothesis, he removed from the surface of a mass of silico-calcareous rock all appearances of crystallization, covered it over with fragments of the same rock, and left it exposed to the air. After a few weeks, some points of rock crystal made their appearance; by degrees the pyramidal summits were formed, then the prism shot out, its bulk diminishing as the crystal became more transparent; and at the end of 23 months six beautiful crystals of quartz had been formed, from $\frac{2}{3}$ to $\frac{3}{4}$ of an inch in length, and $\frac{1}{3}$ of an inch in diameter, the stone around them being proportionally excavated. Encouraged by this success, he procured several specimens of a compound rock, consisting of alalite, garnet, green idocrase, pyroxene, and amorphous pyrites, arranged them in a heap on the chimney-piece of his room, keeping them duly moistened, and after several weeks had the satisfaction of seeing crystals of all these substances emerge from the mass: first came forth small crystals of pyroxene, next summits of alalite, then planes of garnet, and lastly crystals of idocrase and of peridot.

March 1.—A letter was read from M. Coquebert Montbert addressed to G. B. Greenough, Esq. V.P.G.S.; in which the writer suggests the propriety of preserving specimens of the earthy matter brought up from the bottom of the sea by the sounding line, and of applying the knowledge thus collected to determine the submarine extent of the different strata.

A paper by Thomas Weaver, Esq. M.R.I.A. entitled Observations on the distinctive Characters of the principal Classes of Rocks, which was commenced at the preceding meeting, was now concluded.

Mr. W. begins his paper by controverting the Wernerian doctrine of the entire absence of mechanical deposits from the class of primary rocks, by showing, from the writings of Saussure and Von Buch, that rocks of decidedly mechanical structure alternate, and are interstratified with clay-slate, one of the primitive rocks, and even with granite, gneiss, and mica-slate. He next examines, and denies, the validity of the arguments by which certain geologists have attempted to evade this difficulty, by referring all supposed primary rocks so circumstanced to the class of transition rocks, and concludes this part of his subject by maintaining, on the evidence adduced, that rocks exhibiting unequivocal marks of a mechanical structure, as breccias and conglomerates, occur, not merely in formations of a secondary nature, but also in those of primary character.

In the second section of his paper, the author maintains that the *overlying* formations of porphyry, granite, sienite, and trap, which have been referred partly to the primary, and partly to the transition, class, do in fact belong to the *newest floetz trap formation*; and this opinion he supports by an examination of the appearances recorded by M. Von Buch in his Travels in Norway; by Professor Jameson, and others, in Scotland; by M. Giesécké, in Greenland; by Mr. Humboldt, and by himself, in Ireland.

WERNERIAN NATURAL HISTORY SOCIETY.

At the first meeting in the present session (Nov. 25, 1815,) the Secretary read a short communication from Mr. Da Costa, on the discovery of native iron in the lead-mines of Leadhills. Specimens of the iron were exhibited, and an analysis by Mr. Da Costa was also read.

On the same day was read a communication from Capt. Brown, of the Forfarshire Militia, describing several new species of shells found by him in Ireland.

At the next meeting (Dec. 16,) a model and description of a new rain-gage, invented by Mr. Kerr, mathematical instrument-maker, were laid before the Society.

As the mouth of the common rain-gage is often projected into an ellipsis, and varies its area according to the inclination of the fall of rain; it must, therefore, be a very inaccurate instrument. The

object of Mr. Kerr's plan is to ascertain what is the real area of the mouth, or surface exposed during the shower, and so to find the true quantity impinged on a given surface. The instrument is also constructed to show the quarter from which the rain comes. To accomplish these ends, he employs two cups, which are so placed that the planes of their mouths make a right angle with each other. The mouth of the one is vertical, and the mouth of the other is horizontal. We have thus all the varieties which can happen between 0° and 90°, or between a perpendicular fall of rain, and one that is blown parallel to the horizon. The cups are each of them connected with a tube, which conveys the water to the recipients below, and the whole is attached to a wind-vane, which turns round upon a strong iron rod. By this means the mouths of the cups are always kept in a proper position, fully exposed to the shower. The iron rod which supports the whole passes through a square hole in the middle of a cistern-frame divided into 16 spaces, and containing two concentric sets of cisterns. The inner cisterns receive the drops which fall from the tube connected with the vertical cup, and the outer cisterns receive the drops which fall from the tube connected with the horizontal cup. As the cisterns remain fixed, and the gage tubes move with the vane, it is evident that the water can only drop into that cistern which happens to be under the end of the tube at the time, so that we can easily tell what way the wind blew during the shower. Thus if we find water in the south cistern, the vane above must have pointed in that direction. The quantity of water found in each cistern is afterwards poured into a graduated glass tube, and an account of the contents kept in a book having a column ruled for each cistern.

In order to find the true surface exposed, it is necessary that we should have the angle at which the rain is impinged on the cups. This angle is found by comparing the *whole* quantity of water in the cisterns of the horizontal gage with the whole quantity of water in the vertical gage. Thus suppose we find an equal quantity in each, then the rain must have fallen at the angle of 45°, for at that angle the cups present equal surfaces. But if we find more water in one set of cisterns than in the other, the rain must have fallen at a greater angle on that set which contains most; and the angle may be found by the help of a table constructed to show the obliquity which corresponds to any given inequality of water in the recipients.

This rain-gage, in Mr. Kerr's opinion, will not only be entertaining to the meteorologist, but also useful to the farmer, who, by a series of observations, may be able to determine more accurately the climate of his farm. It will point out to him what places require to be most secured, when he is constructing places of shelter for cattle, hay, or corn-stacks, planting trees, and many other rural operations.

At the meeting of Jan. 6, Dr. Macknight read a mineralogical description of Ravensheugh, on the west of East Lothian. It is

composed of red sand-stone, clay-iron-stone, and red marl, which are associated with clink-stone, clink-stone-porphyry, basalt, and trap-tuff.

Jan. 20.—The Secretary read a paper by Dr. Grierson, entitled, Mineralogical Observations in Galloway. It was shown that there are three principal granite masses in Galloway, all of them situated in districts principally composed of transition rocks. In this paper the Doctor described the upper, or Loch Doon, granite. This mass of granite appears to bear the same relation to the stratified country which he formerly found the middle, or Dee, mass to have. The grey-wacke or grey-wacke-slate were nowhere observed in immediate contact with the granite, but every where separated from it by a kind of compact gneiss. The strata of this rock observe the usual direction, not varying above four or five points, and their ends on the N. E. side of the granite run directly towards it. On the E. side of the granite they meet it in a conformable position, and are either nearly vertical, or dip from it. They are much more highly inclined than those which meet with the Dee granite. The Doon granite is in general of the same texture with that of the Dee. But there are two peculiarities with respect to its relation to other rocks; one, its containing numerous and large apparent fragments of gneiss; the other, the occurrence of beds of felspar-porphyry in it.

Feb. 3.—Mr. Campbell, of Carbrook, read a paper on the upright growth of vegetables. After stating the grounds on which he concludes that gravitation is the principle to which perpendicularity is to be ascribed, and examining the hypothesis of Mr. Knight, as to the mode in which that principle operates (which he conceives to be erroneous), Mr. Campbell proposed a theory founded on the law of resisted attraction, by which he considers all *ascents* from the centre to be regulated. The evaporation which constantly accompanies vegetation, and the buoyancy produced by the formation of gaseous vapour, afford, according to his view, a field for the operation of this law; and although some effect may be produced by the upward pressure of hydrogen in a free state, he attributes to the buoyancy of gaseous vapour the chief agency in the upright growth of vegetables.

At the same meeting the Secretary read a communication from James Wilson, Esq. containing remarks on the characters of the *emberiza cirlus* of Linnæus, accompanied with a specimen of the bird shot near Edinburgh.

Feb. 17.—Professor Jameson read a communication transmitted by Mr. Scott, Receiver-General of the Isle of Man, concerning the skull and horns of an extinct species of elk found in a turf bog in that island. The horns differ considerably from those of the Irish elk.

On the same day a communication from Dr. H. E. Holder was read, concerning the effect of the juice of the papaw-tree (*carica papaya*) of the West Indies, in lessening the cohesion of muscular

fibre. Residents in the West Indies avail themselves of this property, to intenerate their butcher's-meat and poultry. For this purpose it has been found that even the exhalation from the stem and leaves is sufficient, and the meat is accordingly hung for about half an hour on a bough of the papaw-tree. The wholesomeness of the meat is not affected by this process. Vauquelin examined the papaw juice, but found nothing remarkable in it; but the subject of its intenerating effects requires to be further inquired into. At present we do not know what share the tropical temperature may have in these results, nor how long after the animal is killed these effects may be produced.

March 2.—There was read the first part of a paper by Mr. Stevenson, civil engineer, on the probability of a change gradually taking place on the level of the German Ocean. In this first part Mr. Stevenson treated only of the wasting effects of the tides upon our shores. He mentioned that in the course of making professional inquiries regarding the impression which the tidal waters of the Frith of Forth are making upon some of the most valuable properties situated upon its banks, he was led to compare these with other observations that have occurred to him in his extensive intercourse with the shores of about one half of Ireland, and the whole coast of Great Britain from Shetland to the Scilly Islands. In doing this he began with the shores of the Frith of Forth, and then proceeded northward along the eastern coast to the Moray Frith, Caithness, and the Orkney and Shetland Islands; next slightly noticed the Lewis, and the western parts of Scotland; then the eastern shores of England, and the British and St. George's Channels. All these places afford many striking examples of the wasting operations of the sea upon the land.

These effects of the sea are not confined to the shores of the German Ocean and the British Channel; for the wasting of the land is no less remarkable in St. George's Channel and the Irish Sea, including the coast of Ireland on the one side, and on the other the shores of Wales, Lancashire, Westmoreland, and the counties of Dumfries, Kirkcudbright, and Galloway, where neither the rocky coasts, and exposed situations of the Islands of Anglesea, Man, Copland, Craig of Ailsa, and the Islands of Cumbrae, nor the sheltered and alluvial shores of the British Channel, are exempted: even the indentations of the coast at Dublin Bay, Liverpool, and Lancaster, and the more extensive Friths of the Solway and the Clyde, are subject to the unvarying destructive effects of the sea upon the land. He concluded that the disintegrating and wearing effects of the waters of the ocean are *general*. Whether we contemplate its effects upon the land by the immediate and powerful impulse of the waves at the base of a rocky shore, or, with the elegant and profound illustrator of the Huttonian theory, trace it in the form of rain, rills, and torrents, in the higher regions, we find its effects all tending to one unvarying principle, the degradation of the land, and consequent tendency to filling up at the bottom of

the sea; while at the same time, from the magnitude and extent of the surface, and other occult causes, we are not aware of the elevation of its level in any sensible degree. That Almighty Being who hath said, "Hitherto shalt thou come, and no further," has, with infinite wisdom, created a kind of compensating power to counterbalance the seeming conflict of the elements of earth and water: for while the ocean appears to be extending its surface, it seems also probable that the quantity of its waters upon the whole are lessened, that part of them undergoes a complete and permanent change of form after the process of evaporation; and that the earthy particles continually accumulating, at least to a certain depth, at the bottom of the sea, have a direct tendency not only to preserve an uniform level, but even in some instances to make the water overrun what we have been accustomed to consider its boundary.

March 16.—There was read an interesting memoir of the late Dr. Walker, Professor of Natural History in the University of Edinburgh, containing an account of his mineralogical studies, and of his systematic arrangement of minerals.

ROYAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Royal Institute of France during the Year 1815.

PHYSICAL DEPARTMENT.—*By M. le Chevalier Cuvier, Perpetual Secretary.*

(*Continued from* p. 315.)

BOTANY.

M. Delabillardiere, who has already published so interesting a work on the plants which he collected in New Holland, when he accompanied the late Entrecasteaux in his expedition, has begun to give an account to the Class of those which he found in New Caledonia. This steep island, uncultivated as it is, and inhabited by unhappy cannibals, produces a great number of fine plants. M. Delabillardiere found in a few days 29 new species of fern, 12 of which are entirely new to the botanist, and have been found nowhere else. The others grow likewise in the other islands of the South Sea; and M. Delabillardiere gives a list of them, to elucidate geographical botany. He arranges these ferns according to the method of Dr. Smith, making some corrections in it. The very accurate figures with which his descriptions are accompanied will give to botanists a complete idea of these important additions to their science.

Every person is acquainted with the aquatic plant called *duckmeat*, and by botanists *lemna*. This moveable and swimming vegetable covers with its green foliage the stagnant waters of almost

every country; but the flowers and fruit of this small and singular plant have not been examined with sufficient care.

M le Baron de Beauvois is the first botanist who has been fortunate enough to collect ripe grains, and to make them germinate. He has followed the lemna thus obtained during its whole progress, and has completed its history, which Ehrhardt and Wolf had only sketched.

It results from the observations of M. de Beauvois that the flower of duckmeat is hermaphrodite, with an envelope which is entire, with two stamina which unfold themselves in succession, with a single style, with a superior ovarium, which becomes an unilocular capsule, splitting circularly at the base, and containing from one to four seeds, which germinate like monocotyledinous seeds, but with very peculiar circumstances, the most remarkable of which is, that the parts considered as the radicle and plumula separate from the first leaf which they produce, and leave it alone to push out roots and other leaves.

Another species of organised being, which covers, and often fills stagnant waters, is the conferva, consisting of a mass of green filaments, sometimes similar to a sort of felt, which some naturalists have wished to assign to the animal kingdom. Their propagation is a good deal different; and some of them, whose filaments are at first disagreeably uniform, swell at intervals, and produce knots from which new filaments proceed. This has induced M. Vaucher to give to these species the name of *proliferæ.* But this botanist warns us not to confound with these filaments springing from the plant itself certain parasitical confervæ which attach themselves to other confervæ, and which have the same appearance.

M. Leclerc de Laval, Member of the Chamber of Deputies, and a very assiduous observer, has presented to the Class a memoir, from which it appears that no other filaments except these parasitical ones exist, and that the propagation of the confervæ, improperly called proliferæ, takes place in the same way as in those called *conjugæ*, by the concentration of the green matter contained in each interval between two cells into an isolated globule which issues from the plant at a certain time, and fixes itself on the first body it meets with in its fall; and, after having thrown out some filaments in order to fix itself, developes a long series of cells.

The author would give to this kind the name of *autarcite*, instead of *proliferæ,* which from his observations is improper. But as M. Desvaux, from other considerations, had given them the name of *cyrtimus*, in a memoir presented more than a year ago, it has been thought unnecessary to introduce a new change in the nomenclature.

M. Henri de Cassini had presented to the Class in 1812 a memoir on the style and stigmata of synanthereæ of plants, usually said to possess a compound flower, and another on their stamina. Towards the end of 1814 he presented a third paper, of which we could not give an account in our last analysis, because the report concerning

it had not been made. It treats of the corollæ of this family of plants.

In this last memoir the author shows that every corolla of a synantherea which is not accompanied by stamina, is monstrous or deformed, so as not to afford any character for the definition of the family or tribe. It follows from this that the semi-flowerets of the semi-flosculous plants, and those of the radiated, have only an apparent analogy, which is not capable of undergoing a severe examination.

He assigns to the corolla of the synanthereæ three principal characters, one of which is very remarkable. It is that each of the five petals, of which he supposes the corolla composed, is furnished with two very simple nerves, which run along its edge from one end to the other on each side, and consequently meet at the bottom; and he attaches to this character so much importance that he proposes to distinguish the family by the name of neuramphipetalæ. Mr. Robert Brown has described this structure in a book published at London in 1814. But M. Cassini had pointed it out before him in unequivocal terms in the second of the memoirs to which we have alluded.

Combining these observations on the corolla with those which he had before made on the style and stigma, and on the stamina, the author divides the family of the synanthereæ into 17 natural tribes, namely, *lactuceæ*, *labiatifloræ* (which he admits with hesitation), *carduaceæ*, *xeranthemeæ*, *echinopsideæ*, *arctotidiæ*, *calendulaceæ*, *heliantheæ*, *ambrosiaceæ*, *anthemideæ*, *inuleæ*, *astereæ*, *senecioneæ*, *tussilagineæ*, *eupatoriæ*, *vernoniæ*. He disposes these 17 tribes, not in a straight line, but in a circular series, which brings the vernoniæ near the lactuceæ.

An unexpected and very curious result of this interesting memoir is, that by the inspection of a single floweret we can almost always determine to what tribe and genus the species which has produced it belongs.

It is to be wished that M. Henri Cassini would speedily publish his researches on the ovarium of the synanthereæ. This will complete the most profound and original examination to which this family has ever been subjected.

M. le Baron la Peyrouse, Professor of Botany at Thoulouse, and Correspondent of the Institute, has given a memoir on four plants of the Pyrenees belonging to the genus *orobus*, of the family of papilionaceous plants. The first of these species had been collected by Tournefort, and called by him *orobus pyrenaicus latifolius nervosus*. It has not again been found alive, and is only known by Tournefort's herbarium, and by those of the botanists of his time. The second, engraved by Plukenet, under the same name, but very different, has always been confounded with that of Tournefort. It is very common in the Pyrenees. After having accurately distinguished these two species by comparative descriptions, M. de la

Péyrouse describes two others quite new, which he found in the same mountains.

M. Desvaux, a botanist of Paris, has endeavoured to subdivide the genera of plants called *cerastium* and *arenaria*, now very numerous, into species. It is chiefly in the greater or less depth of the divisions of the capsule, in the greater or less dilatation of the bases of the stamina, and in some other analogous circumstances, that he thinks he has found characters sufficient to establish the division which he proposes.

Another more general undertaking of the same botanist had for its object the large class of cruciform plants, so remarkable for the uniformity of its structure, and for the great utility of many of its species. In the division of siliculosa he has already established 12 new genera.

M. Kuhnt, a Prussian botanist, has undertaken a new classification of gramina, according to the recent labours of Messrs. de Beauvois and Robert Brown. He makes 12 tribes of them, each founded on a great many characters, such as the number of the styles and stamina, the disposition of the epillets, the number of flowers of each, the consistence and the structure of the glumes and vaginæ.

It is easy to see that such distributions must be studied in the works themselves, and that the most copious analysis would only give an imperfect notion of them. We shall, therefore, satisfy ourselves with having pointed them out.

It has long been supposed by cultivators that the neighbourhood of the barberry is injurious to wheat, and communicates, or at least favours, that species of disease called *rust*. Philosophers have been in the habit of deriding this opinion of cultivators.

M. Yvard, our associate, at once a farmer and philosopher, has rather chosen to determine the fact by experiment than to embrace blindly the one or the other opinion; and his observations, though not yet decisive, rather incline him to the opinion hitherto considered as a prejudice. Wheat planted round a barberry bush was rusted, while the rest of the grain in the same enclosure remained untouched; nor could M. Yvard find any other cause for this difference than the presence of the suspected plant.

Unfortunately, it may be objected that whole districts exist without barberry, which, however, are not exempt from this disease.

Another troublesome disease of corn is the cockspur (ergot), or that long and pointed production which often comes in place of the grain of rye, and other species of corn. M. Decandolle, Professor at Montpellier, and Correspondent, has presented to the Class a memoir, in which he endeavours to prove that the cockspur is a fungus of the genus sclerotium, which assumes nearly the form of the grain, because at first it is moulded in the envelope of the grain. Its substance is analogous to that of the other sclerotiums. Its growth, like that of all the fungi, is favoured by humidity. Its

chemical nature is more similar to that of the fungi than to the seeds of corn. Its smell, likewise, its taste, and its poisonous properties, agree with its fungous nature. It is known that bread made from blighted wheat occasions serious diseases; among others, the dry gangrene, so well known in Sologne, is ascribed to it. M. Decandolle, aware of the importance of destroying so dangerous a production, or at least of diminishing its propagation, conceives that this object would be attained by obliging the proprietors in countries subject to the disease to furnish annually a measure agreed upon which should be burnt upon the spot.

This skilful botanist, who has already derived so much advantage from the study of the aberrations of ordinary forms to elucidate the theory of botany, has employed himself, under this point of view, with those brilliant monstrosities called double flowers. Their production is usually ascribed to the transformation of stamina into petals; but M. Decandolle shows that the transformation or multiplication of different other parts of the flower may equally contribute to it. The pistils, for example, change into petals in certain varieties of anemonies. The stamina themselves may be transformed either by their threads or their antheræ; and it is thus that aquilegia furnishes florists with two sorts of double flowers quite different: and as these two ways of doubling take place only in flowers which have two kinds of petals in the natural state, the author draws from thence a new proof of his assertion that the petals of plants are not specious organs, but only a certain state of the stamina. He points out another kind of double flower, produced by the organs transforming themselves, not into plain petals, but into bundles of petals. This happens most frequently in the families in which the corollæ in the natural state show marks of doubling, as in the pinks. He next examines those flowers in which the alteration of the organs does not amount to a complete transformation, but greatly increases the bulk of certain coloured parts, as happens in hortensia and guelder rose (viburnum opalus). Applying to these different metamorphoses names analogous to those which M. Haüy gives to the different varieties of crystals, he brings them, notwithstanding their apparent irregularity, under certain laws, and a precise nomenclature.

M. de Beauvois, wishing to prevent the fatal accidents so often occasioned by the ignorance of the common people of the qualities of different fungi, has composed a manual for the use of those who are fond of mushrooms, in which he describes, in a language intelligible to every one, the species of this plant which may be eaten without danger, and points out the necessary precautions even with the most innocent of them to prevent them from occasioning any inconvenience; but the most certain rule is only to eat mushrooms raised in beds, and not to eat too many of them.

M. de Mirbel has published the Elements of Vegetable Physiology and of Botany, in two volumes, with a volume of plates. All the important facts respecting the anatomy of plants, their func-

tions, their products, and the difference in the structure of their different parts, is explained with clearness, and elucidated by a great number of fine figures, drawn by the author himself with that skill which he is known to possess. The very copious botanical nomenclature is there explained, and the explanations illustrated by examples. We find, likewise, an interesting history of the science, and of those who have most advanced it. The work is terminated by tables of the principal systems, and particularly by a new exposition of the characters of the natural families of plants.

Article XI.

Scientific Intelligence; and Notices of Subjects connected with Science.

I. *Lectures.*

The Summer Course of Lectures at the Theatre of Anatomy, Medicine, &c., Blenheim-street, Great Marlborough-street, will begin on Monday, June 3, 1816. Anatomy, Physiology, and Surgery by Mr. Brookes, daily at seven in the morning. Dissections as usual. Chemistry and Materia Medica daily at eight in the morning. Theory and Practice of Physic at nine; with examinations by Dr. Ager, Fellow of the Royal College of Physicians, &c. Three courses are given every year, each occupying nearly four months. Further particulars may be known from Mr. Brookes at the Theatre, or Dr. Ager, 69, Margaret-street, Cavendish-square.

Dr. Clutterbuck will begin his Summer Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, early in June.

II. *Cure of Hydrophobia.*

Many of my readers are probably aware that bleeding was successfully employed in a case of hydrophobia in India. It was carried (if we recollect right) the length of producing syncope. Professor Hufeland has lately announced that the same remedy has been tried in different instances in Germany, and that it has been equally successful. He promises to publish some of the cases.

III. *Extraordinary Preservation of Animal Life without Food.*

The following very extraordinary fact is published in the Linnæan Transactions, vol. xi. p. 419, on the authority of Thomas Mantell, Esq. A hog was buried in its stye by a fall of part of the chalk cliff under Dover Castle, Dec. 14, 1810. On the 23d of May, or 160 days after the accident, Mr. Mantell was told that some of the workmen employed in removing the fallen chalk had heard the whining of a pig. He encouraged them, in consequence, to clear away the chalk from the stye under the direction of the owner, Mr.

Poole, who was present. He was soon afterwards surprised to see the pig alive extricated from its confinement. Its figure was extremely emaciated, having scarcely any muscles discernible; and its bristles were erect, though not stiff, but soft, clean, and white. The animal was lively, walked well, and took food eagerly. At the time of the accident it was fat, and supposed to have weighed about 160 lbs; but it now weighed only 40 lbs. Mr. Mantell was assured that at the time of the fall there was neither food nor water in the stye, which is a cave about six feet square, dug in the rock, and boarded in the front; and the whole was covered about 30 feet deep in the fallen chalk. The door and other wood in front of the stye had been much nibbled, and the sides of the cave were very smooth, having apparently been constantly licked for obtaining the moisture exuding through the rock. There was no doubt that some of the loose chalk in front had been eaten; and from the appearance of the excrement, it may be conjectured that it had passed more than once through the intestines.

IV. *French Academy of Sciences.*

By a Royal Edict, dated the 26th of March, 1816, the First Class of the French Institute resumes the name of the Royal Academy of Sciences. It preserves the organisation and distribution in sections of the First Class of the Institute. It is composed as follows:

Sect. 1. *Geometry.*—MM. le Comte Laplace, le Chevalier Legendre, Lacroix, Biot, Poinsot, Ampere.

Sect. 2. *Mechanics.*—MM. Perier, de Prony, le Baron Sané, Molard, Cauchy, Breguet.

Sect. 3. *Astronomy.*—MM. Messier, Cassini, Lefrançais-Lalande, Bouvard, Burckhardt, Arago.

Sect. 4. *Geography and Navigation.*—MM. Buache, Beautemps-Beaupré, Rossel.

Sect. 5. *General Physics.*—MM. Rochon, Charles, Lefevre-Gineau, Gay-Lussac, Poisson, Girard.

Sect. 6. *Chemistry.*—MM. le Comte Berthollet, Vauquelin, Deyeux, le Comte Chaptal, Thenard, Thenard.

Sect. 7. *Mineralogy.*—MM. Sage, Haüy, Duhamel, Lelievre, le Baron Ramond, Brogniard.

Sect. 8. *Botany.*—MM. de Jussieu, de Lamarck, Desfontaines, Labillardiere, Palissot-Beauvois, Mirbel.

Sect. 9. *Rural Economy.*—MM. Tessier, Thouin, Huzard, Silvestre, Bosc, Yvart.

Sect. 10. *Anatomy and Zoology.*—MM. le Comte Lacépède, Richard, Pinel, le Chevalier Geoffroy-Saint-Hilaire, Latreille, Dumenil.

Sect. 11. *Medicine and Surgery.*—MM. le Chevalier Portal, le Chevalier Halle, le Chevalier Pelletan, le Baron Percy, le Baron Corvisart, Deschamps; le Chevalier Delambre, Perpetual Secretary

for the Mathematical Sciences; le Chevalier Cuvier, Perpetual Secretary for the Physical Sciences.

V. *Fermentation.*

Gay-Lussac conceives that during fermentation sugar is converted into nearly equal weights of alcohol and carbonic acid. His reasoning is as follows: sugar, he conceives, is composed by weight of 0·4 carbon and 0·6 water, or of

1 volume vapour of carbon
1 volume vapour of water

or of...... 1 volume vapour of carbon
1 volume of hydrogen
½ volume of oxygen.

Alcohol is composed of

$$1 \text{ volume of olefiant gas} = \begin{cases} 2 \text{ volumes vapour of carbon} \\ 2 \text{ volumes hydrogen} \end{cases}$$

$$1 \text{ volume vapour of water} = \begin{cases} 1 \text{ volume hydrogen} \\ \tfrac{1}{2} \text{ volume oxygen.} \end{cases}$$

Let us triple all the elements of sugar to render the quantity of hydrogen the same in both, it will then be

3 volumes vapour of carbon
3 volumes of hydrogen
$\frac{3}{2}$ volumes of oxygen.

It is obvious that in order to convert sugar into alcohol we must withdraw

1 volume of the vapour of carbon
1 volume of oxygen gas,

which, by combining, form 1 volume of carbonic acid. If we reduce these volumes to weights, we find that 100 parts of sugar are converted by fermentation into 51·34 alcohol and 48·66 carbonic acid. (See Annales de Chimie, xcv. 317.

VI. *Composition of Gum Tragacanth.*

According to Bucholz, gum tragacanth is composed of

Gum	57
Jelly	43
	100

The jelly is the substance which makes tragacanth swell when it is put into water. The gum is soluble in cold water, but not the jelly; but the jelly dissolves in boiling water, and then loses the property of gelatinizing.

VII. *Method of obtaining pure Sulphate of Manganese.*

I am not sure that Fischer's mode of obtaining this salt in a state of purity is known to British chemists. On that account I shall

state it here; because it is easy, and by far the cheapest mode with which I am acquainted. Take one part of green sulphate of iron and four parts of black oxide of manganese; reduce both to powder, and mix them well together. Then expose the mixture to a red heat in a crucible. The dry mass, when digested in water, lets a sulphate of manganese dissolve which is entirely free from iron and copper. Care must be taken that the oxide of manganese employed be free from lime; otherwise the quantity of sulphate of manganese obtained will be small in proportion to the quantity of that earth present.

VIII. *Native Carbonate of Strontian.*

Some time ago I dissolved 3 oz. 400 gr., or 1840 gr., of the native carbonate from Strontian, in Argylshire, in nitric acid, and separated the nitrate of strontian by crystallization. After having separated three or four different crops of crystals, there remained behind a mother liquid, from which I could procure no more nitrate of strontian, either by spontaneous crystallization or evaporation. I therefore evaporated the whole to dryness, and digested the dry mass in alcohol. The alcoholic solution being filtered, a white powder remained on the filter, which was nitrate of strontian. On being dissolved in water, and evaporated, it yielded crystals of pure nitrate of strontian to the very last drop. The alcoholic solution was evaporated to dryness, and re-dissolved in water. The colour of the solution was yellowish brown; but the tinge, I conceive, was owing to part of the alcohol having been altered by the heat; for ammonia threw down nothing from the liquid, nor altered its colour. Carbonate of soda threw down 26 grains of carbonate of lime; but the liquid remained as deeply coloured as ever.

It appears from this experiment that native carbonate of strontian contains a portion of carbonate of lime, either mixed or in combination. As 1840 grains yielded 26 grains of carbonate of lime, it follows that in the native carbonate of strontian there is contained 1·41 per cent. of carbonate of lime. During the evaporation of the alcoholic solution, a portion of it was accidentally spilled, by a sudden motion of the sand-bath on which it was evaporating. Hence the quantity of carbonate of lime was greater than I found; perhaps it amounts nearly to two per cent. Thus it appears that native carbonate of strontian contains as much carbonate of lime as arragonite does of carbonate of strontian.

IX. *Weight of an Atom of Strontian.*

The weight of an atom of strontian which I gave in my table (*Annals of Philosophy*, vol. ii. p. 46), being founded on experiments made with native carbonate of strontian, cannot be quite correct. I thought it, therefore, proper to make an experiment with artificial carbonate, which I knew to be pure. I dissolved 600 grains of nitrate of strontian in water, and precipitated it by carbonate of soda. The precipitate, after being well washed and dried, weighed 300·8 grains of carbonate of strontian. 100 grains of this

carbonate were dissolved with the proper precautions in nitric acid. This experiment was twice repeated. The loss of weight each time was 29·9 grains. Therefore carbonate of strontian is composed of

Carbonic acid	29·9 or 1 atom
Strontian	70·1 or 1
	100·0

Hence it follows that an atom of strontian weighs 6·449. I consider this result as agreeing sufficiently with Klaproth's experiments. He found that the native carbonate lost 30 grains when dissolved in an acid. Now as this mineral contains a mixture of carbonate of lime, it ought to lose more weight when dissolved in an acid than pure carbonate of strontian; for carbonate of lime contains 43·2 per cent. of carbonic acid. If we suppose my experiments correct, and native carbonate of strontian to contain 1·4 per cent. of carbonate of lime, it ought, when dissolved in an acid, to lose 30·086 grains in weight. Now this almost agrees with the experiment of Klaproth. The difference does not amount to $\frac{1}{300}$ part.

X. *Number of Plants known.*

According to Humboldt, the species of plants at present known amount to 44,000. Of these, 6,000 are cryptogamous; the remaining 38,000 have flowers. The distribution of these 38,000 phanerogamous plants is, according to Humboldt, as follows:—

Europe	7,000
Temperate regions of Asia	1,500
Asia, within the tropics and islands	4,500
Africa	3,000
Both temperate regions of America	4,000
America between the tropics	13,000
New Holland, and islands in the Pacific ..	5,000
	38,000

The plants described by the Greeks, Romans, and Arabians, scarcely amounted to 1,400. (Humboldt's Nova Genera et Species Plantarum, Prolegomena, p. 11.

XI. *Verdigris.*

Hitherto verdigris has been chiefly manufactured in France; but a manufactory of it has lately been established at Deptford, near London. I had the curiosity to examine a portion of this verdigris, that I might be able to compare it with the French, the composition of which was already known to chemists from the experiments of Proust.

100 grains of the verdigris being digested for some time in about a pint of distilled water, the whole was thrown upon a filter. I was surprised at the length of time requisite to allow the liquid portion to pass through the filter. At least a fortnight elapsed before

I was able in this way to separate completely the soluble part of the verdigris from the insoluble. The insoluble portion, being dried in the open air, weighed 54·3 grains: of course 45·7 grains had been dissolved.

The solution consisted entirely of acetate of copper, and was composed of

Black oxide of copper	20
Acetic acid	12·85
	32·85

Hence it is obvious that this portion of acetate of copper in the verdigris was combined with 12·85 grains of water.

The insoluble portion, which weighed 54·3 grains, consisted chiefly of subacetate of copper, but contained likewise some carbonate of copper. This carbonate I found could be separated from the subacetate by means of diluted sulphuric acid; for the carbonate is much more soluble in this acid than the subacetate. The analysis made in this way, however, cannot entirely be depended upon, as it somewhat overrates the quantity of carbonate of copper; because the whole dissolved in the sulphuric acid during the effervescence is considered as carbonate, though part of it probably is subacetate. My analysis gave the following quantities:—

Subacetate of copper	23·36
Carbonate of copper	12·10
Water	18·84
	54·30

By dissolving 100 grains of verdigris in water by means of sulphuric acid, and throwing down the copper from the solution by a cylinder of zinc, I obtained 38 grains of copper very nearly. Mr. Benicke informed me that 14,107 lbs. of copper were converted in his manufactory into 41,830 lbs. of verdigris. Hence the copper in 100 grains of verdigris ought to be only 33·72 lbs. From this it is obvious that the verdigris loses weight by keeping. The portion which I examined had stood for some time wrapped up in paper in my laboratory, and probably lost a portion of its weight before I examined it; so that the quantity of water which I found in it, though considerable, was not the whole which it had contained when originally manufactured. French verdigris, according to the analysis of Proust, contains

Soluble acetate of copper	56
Insoluble subacetate	44
	100

XII. *Iolite.*

This is a mineral which was first formed into a peculiar species by Werner. It was afterwards described by Cordier in the Journal de Physique; and Haüy gave it the name of *dichroite*, because it

exhibits one colour when viewed by reflected light, and another when seen by transmitted light. Lucas, in the second volume of his Tableau des Espaces Minerales, proposes to call it *cordierite*, on the supposition (which is ill founded) that Cordier was the discoverer of it. Karsten gave it the name of *iolite*, from its violet colour. This mineral has hitherto been found only in the kingdom of Granada, in Spain, in two places. 1. At Granatillo, near Nijar, disseminated in a blue clay contained in green-stone. 2. At the bay of St. Pedro, in what Cordier describes as a lava.

Its colour is violet-blue, approaching to black. Its primitive form is a six-sided prism; but it occurs likewise in twelve-sided prisms, and in grains. Fracture sometimes imperfectly foliated, sometimes imperfectly conchoidal. Lustre, vitreous. Sometimes opake; sometimes translucent on the edges. Specific gravity 2·560. Before the blow-pipe it melts, with difficulty, into a greyish-green enamel.

It has been lately analyzed by Gmelin, who found its constituents as follows:—

Silica	42·6
Alumina	34·4
Magnesia	5·8
Lime	1·7
Oxide of iron	15·0
Oxide of manganese	1·7
	101·2

The mineral from India known by the name of *saphir d'eau*, which has an indigo-blue colour, and the specific gravity of from 2·555 to 2·670, has likewise been analyzed by Gmelin. He found its constituents as follows:—

Silica	43·6
Alumina	37·6
Magnesia	9·7
Lime	3·1
Potash?	1·0
Oxide of iron	4·5
Oxide of manganese	Trace
	99·5

From this analysis it seems to follow that the saphir d'eau is an iolite.

XIII. *Blue Mineral from Vesuvius.*

This mineral was first noticed by Breislak, in his Voyages dans la Campanie. Bruun Neergaard considered it as a variety of hauyne (Jour. de Mines, No. 125); and in Lucas's Tableau des Espaces Minerales, vol. ii. p. 226, it is classed under the species hauyne.

Its colour is that of ultra-marine, with a shade of grey. Its powder has a light blue colour.

It occurs in plates about two lines in thickness. Lustre, dull; sometimes glimmering. Fracture, even, earthy. Opake. Semi-hard. Does not scratch glass. Has a strong clayey taste.

The following table exhibits the result of the various analyses that have been made of the lazulite, of ultra-marine, of hauyne, and of the blue mineral from Vesuvius.

	Lazulite.		Ultra marine.	Blue mineral from Vesuvius.	Hauyne.	
	*	†	‡	§	‖	**
Silica	46·0	49·0	35·8	47·1	30·0	35·38
Alumina	14·5	11·0	34·8	18·5	15·0	18·87
Magnesia	—	2·0	—	—	—	—
Lime	18·3	16·0	1·6	5·4	15·4	11·62
Potash	—	8·0	—	6·4	11·0	15·45
Soda	—	Trace	23·2	—	—	—
Oxide of iron	3·0	4·0	—	13·7	1·0	1·16
Oxide of manganese	—	—	—	Trace	—	—
Sulphuric acid	3·7	2·0	—	1·2	11·6	12·77
Sulphureted hydrogen	Trace	Trace	3·1	1·0?	Trace	Trace
Carbonic acid	12·5	—	1·5	1·0?	—	—
Water	2·0	Trace	—	Trace	—	1·00
Totals	100	92·0	100	94·3	87	96·35

From the preceding analysis, supposing it accurate, it is obvious that the blue mineral from Vesuvius is specifically different from hauyne. Besides, the external characters, as far as they can be made out from the preceding imperfect description, which I have translated from Leopold Gmelin, show us that the two minerals do not resemble each other.

XIV. *Anhydrite from Ilefeld, in the Harz.*

Stromeyer has lately analyzed an anhydrite from Ilefeld, in the Harz, and found its constituents as follows :—

Lime	40·673
Sulphuric acid	55·801
Carbonic acid	0·087
Oxide of iron	0·254
Silica	0·231
Bitumen	0·040
Water	2·914
Common salt	Trace
	100·000

* Klaproth, Beitrage, vol. i. p. 196.
† Gmelin, Schweigger's Journal, vol. xiv. p. 331.
‡ Clement and Desormes, Ann. de Chim. vol. lvii. p. 317.
§ Gmelin, Schweigger's Journal, vol. xiv. p. 325.
‖ Vauquelin, Jour. de Mines, No. cxxv. p. 365.
** *Annals of Philosophy*, vol. iv. p. 193.

Or of	Anhydrous sulphate of lime	85·877
	Hydrous sulphate of lime	13·400
	Carbonate of lime	0·198
	Oxide of iron	0·254
	Silica	0·231
	Bitumen	0·040
	Common salt	Trace
		100·000

XV. *Indurated Carbonate of Magnesia.*

This variety of *native magnesia*, as the species has been improperly named, was first observed by Haussmann at *Baumgarten*, in Silesia. It was at first taken for lithomarge. Its properties are as follows:—

The colour of the fresh fracture is snow-white. Pieces that have been exposed to the weather have a yellowish-white colour, passing into ochre-yellow. Fracture, fine granular, uneven; here and there passing into splintery, and even. Fragments indeterminate, and sharp edged. Lustre, dull. Does not acquire any lustre, though scratched with the nail. Scarcely translucent on the edges. Very difficultly frangible, and difficult to reduce to powder. Scratches fluor spar and glass, and gives weak sparks with steel. Does not adhere to the tongue. Specific gravity 2·95.

According to the analysis of Stromeyer, it is composed of

Magnesia	47·6334
Carbonic acid	50·7643
Oxide of manganese	0·2117
Water	1·3906
	100·0000

This analysis would make the weight of an atom of magnesia 2·581, which is a little higher than the weight resulting from Berzelius's analysis of sulphate of magnesia.

Article XII.

New Patents.

William Adamson, St. George's, Hanover-square, London; for a principle by which a horizontal wheel may be so moved about its axis by water as to give it a power considerably greater than can be obtained by the application of water to a wheel in any other position. Dec. 22, 1815.

William Plenty, Newbury, Berks, iron-founder; for a plough or agricultural implement, made on an improved principle, answer-

ing a two-fold purpose, so that land may be both pared and ploughed. Dec. 22, 1815.

John Millington, of Duke-street, Manchester-square, engineer; for certain machinery to be moved by wind, steam, manual labour, or any of the processes now employed for moving machinery; by means of which, boats, barges, and other floating vessels, may be propelled or moved in the water. Feb. 1, 1816.

John Budgem, of Dartford, Kent, paper-maker; for a process for reducing rags or articles composed of silk, linen, or cotton, after they have been used, and bringing them into their original state, and rendering the material of which they are composed fit to be re-manufactured, and again applied to beneficial and useful purposes. Feb. 3, 1816.

John George Druke, of Chapman-street, Pentonville, chemist; for a method of expelling the molasses of syrup out of refined sugars, in a shorter period than is at present practised with pipe-clay. Feb. 3, 1816.

William Baynham, of London-road, Surrey, chemist; for a composition for making leather and other articles water-proof. Feb. 20, 1816.

Joseph Manton, of Davies-street, Berkeley-square, gun maker; for improvements in the construction and use of certain parts of fire-arms, and also of the shoeing of horses. Feb. 29, 1816.

Francis Turrel, of Long Acre, coachmaker; for a wheel-guard. March 2, 1816.

George Frederick Muntz, of Birmingham, roller of metals; for a method of abating, or nearly destroying smoke, and of obtaining a valuable product therefrom. March 2, 1816.

John Wood, the younger, of Bradford, worsted spinner, and Joshua Wordsworth, of Leeds, machine-maker; for improvements in machineries applicable to spinning. March 2, 1816.

Bryan Donkin, of Grange-road, Bermondsey, in Surrey, engineer; for a mean or method of effecting certain purposes or processes in which a temperature above that of boiling water is requisite or desirable, by applying the temperature requisite or desirable in the said process for effecting the said purposes in a new manner. March 2, 1816.

John Leigh Bradbury, of Gloucester, gentleman; for improvements in the machinery for spinning of cotton, flax, wool, tow, worsted, or any other fibrous substance. March 9, 1816.

P. F. Montgolfier, of Leicester-square, Middlesex, engineer, and H. D. Dayme, of the same place, gentleman; for improvements in a machine, which acts by the expansion or contraction of air heated by fire; and which machine is applicable to the raising of water, or giving motion to mills or other machines. March 14, 1816.

Pierre François Montgolfier, of Leicester-square, engineer; for improvements on the machine denominated *Bellier Hydraulique*, or *Hydraulic Ram*. March 14, 1816.

William West and Daniel West, both of Bombay; for certain methods of producing and applying power and motion to presses and other mechanical apparatus. March 14, 1816.

James Dawson, of the Strand, Esq.; for new or improved means of producing or communicating motion in or unto bodies, either wholly on in part surrounded by water or air, or any or either of them, by the re-union of suitable apparatus upon the water or air, or upon both of them. March 14, 1816.

Enoch Tonkin, of the City-road, Middlesex; for a globe reflecting stove for light and heat. March 20, 1816.

John Fitkin, of Old street-road, Shoreditch, truss maker, William Fitkin, of the same place, truss maker, and Joseph Barton, of Lombard-street, gentleman; for a new truss. March 23, 1816.

Article XIII.

Scientific Books in hand, or in the Press.

Mr. Donovan has now in the Press an Essay on the Origin, Progress, and present State of Galvanism. It is divided into three parts. The first part contains a sketch of the History of Galvanism, divided into four periods; the second, investigations experimental and speculative of the principal hypotheses; viz. of those of Volta, Fabroni, and of the British Philosophers; of the hypothesis of electro-chemical affinity, as maintained by Davy and Berzelius; and of the identity of the agent in galvanic and electric phenomena. The third part contains a statement of a new Theory of Galvanism; and is divided into two chapters.

In this theory, the agency of an electric or a galvanic fluid is not admitted; the phenomena are conceived to be explicable by the mere operation of chemical affinity.

Mr. Holmes is about to publish a Treatise on the Coal-Mines of Durham and Northumberland, containing Accounts of the different fatal Explosions which have taken place within the last twenty Years, and the means proposed for their remedy; illustrated by Plates of Safety Lamps, &c.

Mr. Weyland's Work on the Principles of Population and Production, as they are affected by the Progress of Society, is just ready for publication.

The Life of the Venerable Antiquarian William Hutton, including a History of his Family, and a particular Account of the Riots at Birmingham in 1791, is about to be published under the Auspices of his daughter.

Mr. Pybus has just ready for Publication a French Grammar, on an entirely new Principle; by which the Language may be acquired or taught in an easy and expeditious manner.

A Translation from the original German, of Professor Morgenstern's Tour in 1809, 1810, through part of Switzerland, Italy, Naples, &c., with Additions, is in the Press.

Article XIV.

METEOROLOGICAL TABLE.

1816.	Wind.	Barometer. Max.	Min.	Med.	Thermometer. Max.	Min.	Med.	Hygr. at 9 a. m.	Rain.	
3d Mo.										
March 20	N W	29·96	29·91	29·935	47	32	39·5			☾
21	E	30·01	29·96	29·985	48	28	38·0	70		
22	S E	30·20	30·01	30·105	53	35	44·0	55		
23	E	30·27	30·20	30·235	48	34	41·0	55		
24	N E	30·27	30·08	30·175	39	34	36·5	58		
25	N E	30·08	30·05	30·065	41	34	37·5	60		
26	N E	30·14	30·08	30·110	42	35	38·5	62		
27	N E	30·14	30·10	30·120	42	33	37·5	53		
28	E	30·12	30·10	30·110	42	33	37·5	47		●
29	E	30·16	30·12	30·140	43	32	37·5	51		
30	E	30·15	30·12	30·135	45	25	35·0			
31	S E	30·15	30·07	30·110	47	26	36·5	61		
4th Mo.										
April 1	S E	30·07	29·80	29·935	50	29	39·5	53		
2	S E	29·79	29·75	29·770	47	29	38·0	49		
3	S E	29·91	29·79	29·850	43	27	35·0	60		
4	E	29·97	29·95	29·960	51	26	38·5	75		
5	Var.	29·95	29·58	29·765	57	29	43·0	55		
6	S W	29·58	29·11	29·345	55	32	43·5	60	·16	
7	W	29·03	28·95	28·990	49	33	41·0	62	·31	☽
8	N E	29·16	29·12	29·140	48	31	39·5	65		
9	N	29·15	29·09	29·120	46	39	42·5	69	·21	
10	S E	29·44	29·34	29·390	55	35	45·0	82	—	
11	N E	29·61	29·44	29·525	50	38	44·0	86	·52	
12	N W	29·70	29·61	29·655	49	33	41·0	60	32	
13	N	29·70	29·49	29·595	40	24	32·0		—	○
14	N W	29·61	29·49	29·550	40	28	34·0	55	3	
15	N W	29·62	29·55	29·585	45	32	38·5	50		
16	N W	29·55	29·38	29·465	51	40	45·5	59	1	
17	N W	29·52	29·38	29·450	56	30	43·0	56		
18	S E	29·64	29·48	29·560	59	36	47·5	52		
		30·27	28·95	29·762	59	24	39·66	60	1·56	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Third Month.—21. Breeze: sunshine. 22. The same. 23. About sun-set, a body of shallow *Cumulostratus*, with an abrupt boundary forward, advanced from the E. 24. Cloudy: breeze. 25. The same. 26. The same. 27. The same. 28. Breeze stronger, unsteady: *Cumulus*. 29. Breeze: *Cumulus* passing to *Cumulostratus*, which cleared off at night, leaving a little *Cirrus* above. 30. a.m. Close *Cumulostratus*, resembling drapery, as frequent in cold spring weather: p.m. more open sky. 31. a.m. Large *Cumuli*: wind S. E. gentle: the temp. was 45° at ten, a.m.: the roads are now dusty to an extreme: *Cirrus* passing to *Cirrostratus* at evening.

Fourth Month.—1. Hoar frost: sunshine: *Cirri*, with haze above. 2. *Cirrostratus*, with *Cirrus*: breeze much stronger. 3. Windy: hoar frost: *Cirrus*. 4. Hoar frost: sunshine: *Cirrus*, with *Cumulus*: drains emit an offensive gas. (This is a very common circumstance after long settled weather, before a change, and depends unquestionably in great measure on a renewed electrical action on the general surface.) 5. White frost: misty from the N.: the wind N. E.: sunshine: at night a lunar halo of the largest diameter: *Cirrostratus*. 6. a.m. The higher atmosphere filling: *Cirrus*, *Cirrocumulus*, &c.: wind N.: a smart breeze: then S. W.: wind and rain in the night. 7. a.m. Dripping: sleet: cloudy: windy: *Cumulostrati*, succeeded by numerous *Nimbi*, letting fall showers of large opake hail, followed by rain: three distinct peals of thunder, p.m.: one N., another S., and a third near at hand, with lightning. 8. Cloudy: windy. 9. Windy at N., and more so in the night, seemingly from the westward: rain. 10. *Cumulostratus*: some dripping: rain by night. 11. a.m. Obscurity early, with *Cirrostratus* beneath to S.: rain and wind chiefly from the N. E.: p.m. moderate weather. 12. Sky as yesterday, but the *Cirrostratus* to N. E.: rain at mid-day. In the night a gale from N.W., with snow for two hours. 13. a.m. The high ground to the W. and N. W. is white with snow: with us none remains. 14. White frost (eight, a.m.), yet cloudy overhead, and a group far to the N., in which were *Nimbi*: in an hour's time this group reached us, and we had showers of heavy granular snow by intervals. 15. Clear morning: dew: fair, though with *Nimbi* in sight: very high tides, and much water out in the marshes. 16. A moderate gale at S. and S. W.: some rain by night. 17. a.m. Cloudy: calm: mild. 18. *Cumulus*, *Cirrus*: sunshine, with cool breeze.

RESULTS.

Winds for the most part Easterly, non-electric, keen, and drying.

Barometer:	Greatest height	30·27 inches.
	Least	28·95
	Mean of the period	29·762
Thermometer:	Greatest height	59°
	Least	24
	Mean of the period	39·66

Mean of De Luc's Hygrometer at 9 a.m. .... 60°

Rain .... 1·56 inch.

The mean temperature of this period is full 8° lower than that of the corresponding portion of 1815. It has accordingly presented a striking contrast to the latter in its effects on the vegetable kingdom; not a single day having occurred in it of that which cultivators emphatically denominate "growing weather," when a moist air co-operates with a rising temperature (perhaps also with an abundant electricity) to stimulate vegetable life, and make way for the unfolding of its products.

Tottenham, *Fourth Month*, 22, 1816. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

JUNE, 1816.

Article I.

Biographical Sketch of Alexander Wilson.

(*Concluded from* p. 342.)

We now approach that era of our author's life in which we behold him emerging from the vale of obscurity, and attaining that enviable distinction in the republic of science and letters which it is the lot of but few to enjoy.

Mr. Samuel F. Bradford, bookseller, of Philadelphia, being about to publish an improved edition of Rees's New Cyclopædia, Mr. Wilson was introduced to him as one qualified to superintend the work; and was engaged, at a liberal salary, as assistant editor.

Not long after this engagement he unfolded his mind to Mr. Bradford on the subject of an American Ornithology, and exhibited such evidence of his talents for a publication of that nature, that Mr. Bradford promptly agreed to become the publisher, and to furnish the requisite funds; and now for the first time Mr. Wilson found those obstructions removed which had opposed his favourite enterprise.

All things being thus happily arranged, he applied himself to his varied and extensive duties with a diligence which scarcely admitted repose; until finding his health much impaired thereby, he was induced to seek the benefits of relaxation in a pedestrian excursion through a part of Pennsylvania, which afforded him a favourable opportunity of procuring specimens of birds, and some additional information relating to them of which he was very desirous to be possessed.

This jaunt was made in the month of August, 1807; and on the return of Mr. Wilson he engaged in his avocations with renewed

ardour, devoting every moment which could be spared from his editorial duties to his great work.

At length, in the month of September, 1808, the first volume of the American Ornithology made its appearance. From the date of the arrangement with the publisher, a prospectus had been issued, wherein the nature and intended execution of the work were specified. But yet no one appeared to entertain an adequate idea of the elegant treat which was about to be afforded to the lovers of the arts and of useful literature. And when the superb volume was presented to the public, their delight was only equalled by their astonishment that our country, as yet in its infancy, should produce an original work in science that could vie, in its essentials, with the proudest productions of a similar nature of the European world.

In the latter part of September Mr. Wilson set out on a journey to the eastward, to exhibit his book and procure subscribers. He travelled as far as the district of Maine, and returned through Vermont, by the way of Albany, to Philadelphia. From a letter to a friend, dated Boston, Oct. 10, 1808, we have made the following extract :—

"I have purposely avoided saying any thing either good or bad on the encouragement I have met with. I shall only say that, among the many thousands who have examined my book, and among these were men of the first character for taste and literature, I have heard nothing but expressions of the highest admiration and esteem. If I have been mistaken in publishing a work too good for the country, it is a fault not likely to be soon repeated, and will pretty severely correct itself. But whatever may be the result of these matters, I shall not sit down with folded hands whilst any thing can be done to carry my point; since God helps them who help themselves. I am fixing correspondents in every corner of these northern regions, like so many piquets and outposts, so that scarcely a *wren* or *tit* shall be able to pass along, from York to Canada, but I shall get intelligence of it."

From several individuals, in this journey, Mr. Wilson experienced the most polite and encouraging attentions; but from others, and those too from whom most was expected, he met a reception of an opposite nature, the rudeness of which we should hesitate to record if the facts were not supported by his own declaration. From his private journal we have taken the following extracts :—

"Arrived at ———; waited on Dr. ———, principal of the seminary. It was near dusk before I could see him; and our conversation, which was held on the steps leading to his house, occupied about five minutes. He considered the volume too expensive for any class of readers about this town. He behaved with cold indifference—turned over a few leaves without any seeming interest; and said, that as far as he could *see* (it was nearly dark) it looked well—returned the volume, and we parted. If, as principal of this college, this literary luminary shed no more cheering influence over

the exertions of his pupils than he did on the author of American Ornithology, I don't much wonder that storms and tempests should desolate this seminary, and damp the energies of its inhabitants."

"Arrived at ———. Called on the Governor at the Health Office; there were several Gentlemen in company. He turned over a few leaves very carelessly, asked some trifling questions, and then threw the book down, saying—"*I don't intend to give an hundred and twenty dollars for the knowledge of birds!*" Taking up a newspaper he began to read. I lifted the book, and, without saying a word, walked off with a smile of contempt for this very *polite* and very *learned* Governor. If science depended on such *animals* as these, the very name would long ere now have been extinct.

"The City Recorder declared that he never read or bought books on animals, fishes, plants, or birds—*he saw no use in them!* Yet this same *reptile* could not abstain from acknowledging the beauty of the plates of my Ornithology."

If Mr. Wilson had been treated with disrespect by the vulgar or illiterate, he would have imputed it to the right cause—a want of breeding. Or if he had been soliciting encouragement to a work of which he was not enabled to afford a specimen, whereby its character could be estimated, there might be some palliation of conduct, which, placed in the most favourable point of view, must still bear the epithet uncivil. But the author of American Ornithology addressed himself to persons of rank and of learning. He modestly asked support equal to his merits; he claimed that deference which is ever due to the gentleman; and, to prove himself no pretender or impostor, he exhibited his *Diploma regium signo majori consignatum*, the unquestionable credentials of Science herself.

Mr. Wilson, after tarrying at home a few days, departed to the southward, visiting every city and town of importance, as far as Savannah, in the state of Georgia. This journey being performed in the winter, and alone, was of course not attended with many travelling comforts; and, to avoid the inconveniences of a return by land, he embarked in a vessel, and arrived at New York in the month of March, 1809. This was rather an unproductive tour, but few subscriptions being obtained.

Of the first volume of the Ornithology only 200 copies had been printed. But it was now thought expedient to strike off a new edition of 300 more, as the increasing approbation of the public warranted the expectation of corresponding support.

The second volume was published in January, 1810; and our indefatigable ornithologist set out for Pittsburg, the latter part of the same month, on his route to New Orleans. After conferring with his friends on the most eligible mode of descending the Ohio, he resolved, contrary to their dissuasions, on venturing in a skiff by himself; this mode, with all its inconveniences, being considered as best suited to his funds, and as most favourable to his researches. Accordingly, on Feb. 24, he embarked in his little boat, and bade adieu to Pittsburg. After a variety of adventures he arrived in

safety at Louisville, being upwards of 700 miles from the place of his departure. Here he disposed of his skiff, and then set out on foot for Lexington, 72 miles further. At this last place he purchased a horse; and being prepared for the long and disagreeable route which lay before him, he resolutely explored his way alone, and safely reached the town of Natchez on May 17, being a distance of 678 miles from Lexington. In his journal he says—"This journey, 478 miles from Nashville, I have performed alone, through difficulties that those who have never passed the road could not have a conception of." We may readily suppose that he had not only difficulties to encounter, encumbered as he necessarily was with his shooting apparatus and increasing baggage, but also dangers, in journeying through a frightful wilderness, where almost impenetrable cane-swamps and morasses present obstacles to the progress of the traveller which require all his resolution and activity to overcome. Added to which, he had a severe attack of the dysentery, when far remote from any situation which could be productive of either comfort or relief; and he was under the painful necessity of trudging on, debilitated and dispirited with a disease which threatened to put a period to his existence. An Indian, having been made acquainted with his situation, recommended the eating of strawberries, which were then fully ripe, and in great abundance. On this delightful fruit, and newly laid eggs, taken raw, he wholly lived for several days; and he attributed his restoration to health to these simple remedies.

Previously to entering the wilderness, Mr. Wilson had the melancholy satisfaction of shedding tears of sorrow at the grave of his friend, the amiable and intrepid Governor Lewis; who, distracted by base imputations and cruel neglect, closed his honourable and useful life by an inglorious act of suicide, in the cabin of a settler named Grinder, and was buried close by the common path, with nothing but a few loose rails thrown over his grave.

On June 6, our traveller reached New Orleans, distant from Natchez 252 miles. As the sickly season was fast approaching, it was deemed advisable not to tarry long in this place; and his affairs being despatched, he took passage in a ship bound to New York, at which place he arrived on July 30, and soon reached Philadelphia, enriched with a copious stock of materials for his work, including several beautiful and hitherto unknown birds.

In the newly settled country through which Mr. Wilson had to pass in his last journey, it was reasonable not to expect much encouragement in the way of subscriptions. Yet he was honoured with the names of many respectable individuals, and received not only civilities, but also kind treatment. From his journal and letters we might select many passages of much interest to the reader; but the limits allotted to this memoir will not admit of copiousness of detail, and we shall content ourselves with two or three extracts.

"In Hanover, Pennsylvania, a certain Judge H. took upon

himself to say that such a book as mine ought not to be encouraged, as it was not *within the reach of the commonality*, and therefore *inconsistent with our republican institutions!* By the same mode of reasoning, which I did not dispute, I undertook to prove him a greater culprit than myself, in erecting a large, elegant, three-story brick house, so much beyond the reach of the *commonality*, as he called them, and consequently grossly contrary to our republican institutions. I harangued this Solomon of the Bench more seriously afterwards, pointing out to him the great influence of science on a young nation like ours, and particularly the science of natural history, till he began to show such symptoms of *intellect* as to seem ashamed of what he had said."

"*March* 23.—I bade adieu to Louisville, to which place I had four letters of recommendation, and was taught to expect much of every thing there; but neither received one act of civility from those to whom I was recommended, one subscriber, nor one new bird, though I delivered my letters, ransacked the woods repeatedly, and visited all the characters likely to subscribe. Science or literature has not one friend in this place."

April 25.—Breakfasted at Walton's, 13 miles from Nashville. This place is a fine rich hollow, watered by a charming clear creek, that never fails. Went up to Madison's Lick, where I shot three paroquets and some small birds.

April 26.—Set out early, the hospitable landlord, Isaac Walton, refusing to take any thing for my fare, or that of my horse, saying, "*You seem to be travelling for the good of the world, and I cannot, I will not, charge you any thing. Whenever you come this way, call and stay with me, you shall be welcome!*" This is the first instance of such hospitality which I have met with in the United States."

"*Wednesday, May* 23.—Left Natchez, after procuring 12 subscribers; and, having received a kind letter of invitation from Wm. Dunbar, Esq., I availed myself of his goodness, and rode nine miles along the usual road to his house, where, though confined to his bed by a severe indisposition, I was received with great hospitality and kindness; had a neat bed-room assigned me, and was requested to consider myself as at home during the time I should find it convenient to stay in exploring that part of the country."

The letter above mentioned, which is now before us, is worthy of transcription:—

"Sir, *Forest, May* 20, 1810.

"It is very unfortunate that I should be so much indisposed as to be confined to my bed-room; nevertheless, I cannot give up the idea of having the pleasure of seeing you as soon as you find it convenient. The perusal of your first volume of Ornithology, lent me by General Wilkinson, has produced in me a very great desire of making your acquaintance.

"I understand from my boy that you propose going in a few days

to New Orleans, where you will see some small cabinets of natural history that may interest you. But as I presume it is your intention to prosecute your inquiries into the interior of our country, this cannot be done better than from my house as your head quarters, where every thing will be made convenient to your wishes. My house stands literally in the forest, and your beautiful orioles, with other elegant birds, are our court-yard companions.

"The bearer attends you with a couple of horses, on the supposition that it may be convenient for you to visit us to-day; otherwise he will wait upon you any other day that you shall appoint.

"I am respectfully, &c.

"WILLIAM DUNBAR."

This excellent Gentleman, whose hospitality was thus promptly excited, has since paid the debt of nature; and his grateful guest fondly cherished to the last hour of his existence the remembrance of those happy moments which were passed in his society, and that of his amiable and accomplished family.

In September, 1812, Mr. Wilson set off to the eastward, to visit his subscribers. In a letter to the editor he writes,—"I coasted along the Connecticut river to a place called Haverhill, ten miles from the foot of Moose-hillock, one of the highest of the White Mountains of New Hampshire. I spent the greater part of a day in ascending to the peak of one of these majestic mountains, whence I had the most sublime and astonishing view that was ever afforded me. One immensity of forest lay below, extended on all sides to the furthest verge of the horizon; while the only prominent objects were the columns of smoke from burning woods, that rose from various parts of the earth beneath to the heavens; for the day was beautiful and serene."

This excursion was succeeded by rather an unpleasant occurrence. The good people of Haverhill perceiving a stranger among them of very inquisitive habits, and who evinced great zeal in exploring the country, sagaciously concluded that he was a spy from Canada, employed in taking sketches of the place, to facilitate the invasion of the enemy. Under these impressions it was thought conducive to the public safety that Mr. Wilson should be apprehended; and he was accordingly taken into the custody of a magistrate, who, on being made acquainted with his character, and the nature of his visit, politely dismissed him, with many apologies for the mistake.

The publication of the Ornithology now progressed as rapidly as a due regard to correctness and elegance would permit. In order to become better acquainted with the feathered tribes, and to observe their migrations with more accuracy, as well as to enjoy the important advantages of a rural retirement, Mr. Wilson resided the better part of the years 1811-12 at the botanic garden of his friend, Mr. Bartram. There, removed from the noise, bustle, and interruption, of the metropolis, he was enabled to dispose of his time

to the best advantage; for when fatigued with close application within doors, to recruit his mind and body he had only to cross the threshold of his abode, and he at once found himself surrounded by those acquaintance, the observance of whose simple manners not only afforded the most agreeable recreation, but who were perpetually contributing to the great uudertaking which he was earnestly labouring to complete.

Besides the journies which have been already mentioned, he made several short excursions to different parts, and was five times at the coast of New Jersey, in pursuit of the waders and web-footed tribes which are there found in immense numbers. The aggregate of his peregrinations amounted to upwards of *ten thousand miles.*

In the early part of the year 1813 the seventh volume of the Ornithology was published; and the author immediately made preparations for the succeeding one, the letter-press of which was completed in the month of August. But unfortunately his great anxiety to conclude the work condemned him to an excess of toil, which, inflexible as was his mind, his bodily frame was unable to bear. He was likewise by this flood of business prevented from residing in the country, where hours of lassitude might have been beguiled by a rural walk, or the rough but invigorating exercise of the gun. At length he was attacked by a disease, which perhaps at another period of his life might not have been attended with fatal effects, but which now, in his debilitated frame and harassed mind, proved a mighty foe, whose deadly assaults all the combined efforts of friendship, science, and skill, could not repel. The dysentery, after a few days' illness, closed the mortal career of Alex. Wilson, on Aug. 23, 1813.

It may not be going too far to maintain that in no age or nation has there ever arisen one more eminently qualified for a naturalist than the subject of these memoirs. He was not only an enthusiastic admirer of the works of creation, but he was consistent in research, and permitted no dangers or fatigues to abate his ardour or relax his exertions. He inured himself to hardships by frequent and laborious exercise; and was never more happy than when employed in some enterprise which promised from its difficulties the novelties of discovery. Whatever was obtained with ease, to him appeared to be attended, comparatively speaking, with small interest: the acquisitions of labour alone seemed worthy of his ambition. He was no closet philosopher—exchanging the frock of activity for the night-gown and slippers. He was indebted for his ideas, not to books, which err, but to Nature, which is infallible; and the inestimable transcript of her works which he has bequeathed us possesses a charm which affects us the more the better acquainted we become with the delightful original. His inquisitive habits procured him from others a vast heterogeneous mass of information; but he had the happy talent of selecting from this rubbish whatever was valuable. His perseverance was uncommon; and when engaged in pursuit of a particular object he would never relinquish it

while there was a chance of success. His powers of observation were very acute, and he seldom erred in judgment when favoured with a fair opportunity of investigation.

That the industry of Mr. Wilson was great his work will for ever testify; and our astonishment is excited that so much should have been performed in so short a time. When we take into consideration the state of our country, as respects the cultivation of science; and that in the walk of ornithology particularly no one *deserving the title of a naturalist* had yet presumed to tread; when we view the labours of foreigners who have interested themselves in our natural productions, and find how totally incompetent they were, through a deficiency of correct information, to instruct; and then when we reflect that a single individual, "*without patron, fortune, or recompence*," has accomplished in the short space of *seven* years as much as the combined body of European naturalists have taken a *century* to achieve, we feel almost inclined to doubt the evidence of our senses. But it is a fact, which we feel a pride in asserting, that we have as faithful, complete, and interesting, an account of *our* birds in the estimable volumes of the American Ornithology, as the Europeans can at this moment boast of possessing of *theirs*. Let those who doubt the correctness of our opinion examine for themselves, and determine according to the dictates of an unbiassed judgment.

We need no other evidence of the unparalleled industry of our author than the fact that of *two hundred and seventy-eight species* which have been figured and described in his ornithology,* *fifty-six* of these have not been noticed by any former naturalist; and several of the latter number are so extremely rare, that the specimens from which the figures were taken were the only ones that he was ever enabled to obtain. The collection and discovery of these birds were the fruits of many months of unwearied research amongst forests, swamps, and morasses, exposed to all the dangers, privations, and fatigues, incident to such an undertaking. What but a remarkable passion for the pursuit, joined with the desire of fame, could have supported a solitary individual in labours of body and mind, compared to which the bustling avocations of common life are mere holiday activity or recreation!

Independently on that part of his work which was Mr. Wilson's particular province, viz. the drawing of his subjects and their histories, he was necessitated to occupy much of his time in colouring the plates; his sole resource for support being in that employment, as his duties as assistant editor of the Cyclopædia had ceased. This is a circumstance much to be regretted, as the work would have progressed more rapidly if he could have avoided that confining drudgery. The principal difficulty, in effect, attending this work, and that which caused its author most uneasiness, was the colouring of the plates. If this could have been done solely by himself; or,

* The whole number of birds figured is 320.

as he was obliged to seek assistance in this delicate process, if it could have been performed immediately under his eye, he would have been relieved of much anxiety,* and would have better maintained a due equanimity, his mind being daily ruffled by the negligence of his assistants, who too often, through a deplorable want of skill and taste, made disgusting caricatures of what were intended to be modest imitations of simple nature. Hence much of his precious time was spent in the irksome employment of inspecting and correcting the imperfections of others. This waste of his stated periods of labour he felt himself constrained to supply by encroachments on those hours which Nature, tenacious of her rights, claims as her own: hours which she consecrates to rest—which she will not forego without a struggle; and which all those who would preserve unimpaired the vigour of their mind and body must respect. Against this intense and destructive application his friends failed not to admonish him; but to their kind regards he would reply that "life is short, and without exertion nothing can be performed." But the true cause of this extraordinary toil was his poverty. By the terms of agreement with his publisher, he was to furnish, at his own cost, all the drawings and literary matter for the work, and to have the whole under his controul and superintendence. The publisher obligated himself to find funds for the completion of the volumes. To support the heavy expense of procuring materials, and other unavoidable expenditures, Mr. Wilson's only resource, as has been stated, was in colouring the plates.

In the preface to the fifth volume he observes, "The publication of an original work of this kind in this country has been attended with difficulties, great, and, it must be confessed, sometimes discouraging to the author, whose only reward *hitherto* has been the favourable opinion of his fellow citizens, and the pleasure of the pursuit.

"Let but the generous hand of patriotism be stretched forth to assist and cherish the rising arts and literature of our country, and both will most assuredly, and that at no remote period, shoot forth, increase, and flourish, with a vigour, a splendour, and usefulness, inferior to no other on earth."

We have here an affirmation that the author had laboured without reward, except what was conferred by inefficient praise, and an eloquent appeal to the *generosity* and *patriotism* of his fellow citizens. Seven illustrious cities disputed the honour of having given *birth* to the Prince of Epic song. Philadelphia first beheld that phenomenon the American Ornithology, rising amidst her boasted opulence, to vindicate the claims of a calumniated portion of creation, and to furnish her literary pride with a subject of exultation for ages to come. Yet duty calls upon us to record a

* In the preface to the third volume Mr. Wilson states the anxiety which he had suffered on account of the colouring of the plates, and of his having made an arrangement whereby his difficulties on that score had been surmounted. This arrangement proved in the end of greater injury than benefit.

fact which may cause our native city to feel the glow of shame. Of all her literati, her men of benevolence, taste, and riches, *seventy* only, to the period of the author's decease, had the liberality to countenance him by a subscription, more than half of whom were *tradesmen, artists*, and those of the middle class of society; whilst the little city of New Orleans, in the short space of *seventeen days*, furnished *sixty* subscribers to the American Ornithology!

Mr. Wilson was possessed of the nicest sense of honour. In all his dealings he was not only scrupulously just, but highly generous. His veneration for truth was exemplary. His disposition was social and affectionate. His benevolence extensive. He was remarkably temperate in eating and drinking: his love of retirement preserving him from the contaminating influence of the convivial circle. And, unlike the majority of his countrymen, he abstained from the use of tobacco in every shape. But as no one is perfect, Mr. Wilson in a small degree partook of the weakness of humanity. He was of the *genus irritabile*, and was obstinate in opinion. It ever gave him pleasure to acknowledge error when the conviction resulted from his own judgment alone, but he could not endure to be told of his mistakes. Hence his associates had to be sparing of their criticisms, through a fear of forfeiting his friendship. With almost all his friends he had occasionally, arising from a collision of opinion, some slight misunderstanding, which was soon passed over, leaving no disagreeable impression. But an act of disrespect, or wilful injury, he would seldom forgive.

Such was Alexander Wilson. When the writer of this humble biography indulges in retrospection, he again finds himself in the society of that amiable individual whose life was a series of those virtues which dignify human nature; he attends him in his wild-wood rambles, and listens to those charming observations which the magnificence of creation was wont to give birth to; he sits at his feet, and receives the instructions of one, in science, so competent to teach; he beholds him in the social circle, and notes the complacency which his presence inspired in all around. But the transition from the past to the present quickens that anguish with which his heart must be filled, who casts a melancholy look on those scenes a few months since graced with the presence of one, united to him by a conformity of taste, disposition, and pursuit; and who reflects that that beloved friend can revisit them no more.

It was the intention of Mr. Wilson, on the completion of his ornithology, to publish an edition in four volumes octavo, the figures to be engraved on wood, somewhat after the manner of Bewick's British Birds, and coloured with all the care that has been bestowed on the original plates. If he had lived to effect such a scheme, the public would have been put in possession of a work of considerable elegance as respects typography and illustrations; wherein the subjects would have been arranged in systematical order, and the whole at a cost of not more than one-seventh part of the quarto edition.

He likewise contemplated a work on the quadrupeds of the United States; to be printed in the same splendid style of the Ornithology; the figures to be engraved with the highest finish, and by the best artists of our country. How much has science lost in the death of this ingenious and indefatigable naturalist!

Mr. Wilson was interred in the cemetery of the Swedish church, in the district of Southwark, Philadelphia. While in the enjoyment of health, he had conversed with a friend on the subject of his dissolution, and expressed a wish to be buried in some rural spot sacred to peace and solitude, where the charms of nature might invite the steps of the votary of the Muses and the lover of science, and where the birds might sing over his grave.

It has been an occasion of regret to those of his friends to whom was confided the mournful duty of ordering his funeral that his desire had not been made known to them, otherwise it should have been piously observed.

Article II.

Chemical Analysis of some Membranous Bodies of Animals. By Professor I. F. John.*

The substances of which I propose to speak in this paper are the epidermis, nails, horns, claws, hoofs, feathers, &c. Though they have been often examined by chemists, as is evident from the great number of examples which I have given in my Tables of the Animal Kingdom, published in Berlin in 1814, yet it will very soon be remarked that there is not a single experiment which fully comes up to our wishes; for all that we at present know is that they consist of an insoluble substance combined with some phosphate of lime. Respecting the nature of this insoluble substance we are still in the dark, and do not know whether, according to the opinion of Fourcroy and Vauquelin, it consists of indurated mucus; or of fibrin, as Scherer and Hildebrant conceive; or of albumen, as Hatchett thinks he has ascertained; or, as I conceive, of modifications sometimes of one, sometimes of another, of these bodies.

As to this last opinion, it will be very difficult to establish it. Indurated mucus, indurated albumen, and animal fibrin, may be distinguished from each other by striking chemical properties when we possess each of them in a state of purity. But the many striking properties which they possess in common, and the passage from the one to the other, so frequent in animal bodies, make it very difficult to distinguish them from each other, and lead to the opinion that they are modifications of the same constituents. When

* Translated from Schweigger's Journal, vol. xiv. p. 302, October, 1815.

in a state of solution, they exhibit quite different properties, and are easily distinguished; but when they become insoluble, they do not appear to undergo merely a coagulation or condensation, but to assume quite different chemical properties. Several facts lead to the conclusion that fibrin contains the greatest proportion of azote; that albumen follows next in this respect, while mucus contains the least. But as these differences are but small, it is difficult to render them sensible by analysis. This character seems likewise to be much influenced by the solvent; for hair, which, according to Vauquelin's analysis, consists almost entirely of mucus, contains, however, not less azote than albumen does. The phenomena which these bodies exhibit when they undergo spontaneous decomposition, their relation to different acids, and to water, the effects of a high temperature, of a dry distillation, &c. may afford marks of distinction to practised chemists; and these are the characters which I employ in my experiments.

It is very much to be desired that chemists would prosecute this subject till accurate characters be ascertained by means of which these three substances may be distinguished from each other; for the advantages resulting from such a discovery would not be confined to chemistry, but would extend likewise to physiology, as consequences might be drawn from it respecting the source of the formation of these matters. But I must not prosecute this subject any further, that I may not appear to deviate from the object which I have in view.

I. *Epidermis of the Foot.*

(*a*) When boiled in water, about five or six per cent. were dissolved. When the concentrated solution was left in the temperature of 77°, it dried to a yellowish, transparent, tough mass, in which a number of small crystals were visible, though they could not be separated.

The concentrated solution acted as an acid on litmus paper. On cooling, it gelatinized very imperfectly. It was precipitated by solutions of mercury, silver, lead, and oxalic acid. Tincture of nutgalls occasioned scarcely any precipitate; and barytes, ammonia, and alcohol, none at all. Lime caused a smell of ammonia to exhale. Hence it contains neither a sulphate nor phosphate of lime. An acid, a trace of gelatin, and mucus, were its principal ingredients.

From the dried mass of the decoction alcohol dissolved, besides an uncombined acid, some salts, which separated in crystals. This acid possessed all the properties of the acid discovered by Scheele in milk, and afterwards by Berzelius in different animal substances, and known by the name of *lactic acid*.

(*b*) The portion of epidermis which is insoluble in water appeared, after the boiling, snow-white, and prodigiously swelled. It dissolved completely in nitric acid, and produced much oxalic acid. When heated, it dried to a very hard skin, which possessed

its natural semi-transparence, and in a higher temperature it fused. It dissolved very speedily in a caustic ley. The boiled epidermis being distilled, gave out, like albumen, first a fetid ammoniacal liquid, then a yellow oil, much concrete salt, and the usual gaseous products, without a trace of an acid. When allowed to putrefy, it exhibited the same appearances as albumen, to which in all its other properties it has the closest resemblance.

(*c*) By incineration about $\frac{1}{4}$ per cent. of a reddish ash was obtained, from which water dissolved a little potash, and sulphate, muriate, and phosphate, of potash. There remained behind a reddish residue, from which nitric acid dissolved a trace of phosphate of lime. What remained consisted of gypsum, with traces of iron, and, as it seemed, of manganese.

Chemists usually suppose, when they find no sulphate of potash among the salts obtained by the moist way, though it constitutes an ingredient of the ash, that this proves the presence of sulphur in the substance under examination. For my part, I am of opinion that to establish such a position much more accurate experiments would be requisite than have hitherto been made. In the present case it is easy to conceive that the presence of sulphate of potash in the ash is owing to the decomposition of gypsum.

(*d*) On treating the epidermis with alcohol, at a temperature between 77° and 100°, only $\frac{1}{2}$ per cent. of a fatty matter was dissolved, which was precipitated by water, and separated by evaporation.

A hundred parts of the epidermis of the human foot are, therefore, composed of the following constituents :—

Indurated albumen	93 to 95
Mucus, with a trace of animal (gelatinous?) matter	5
Lactic acid Lactate of potash Phosphate of potash Muriate of potash Sulphate of lime Ammoniacal salt Phosphate of lime Manganese? and iron	1
Soft fat	0·05

Observations.

From this analysis, it follows that Hatchett's determination, that it consists of indurated albumen, is correct. It is very probable that the epidermis is formed from the lymph contained in the lymphatic vessels that pass through the skin. By strong friction, as takes place in several mechanical handicrafts, by much walking, &c. these vessels are probably ruptured in great numbers. Hence the great thickening of the cuticle which takes place in such cases.

The fatty matter contained in the epidermis seems intended to

keep the surface always moist and smooth. It is this matter which in summer frequently makes its appearance in considerable quantity in the sweat as a liquid oil. In some diseases, in which the nails, skin, &c. become brittle, this fatty matter seems to be wanting, or to be diminished in quantity.

II. *Epidermis from the Arm of a Woman who was afflicted with Herpes.*

This woman had formerly been afflicted with different diseases, and was probably labouring under phthisis. More lately she had a scaly eruption, possessing the characters of herpes. It occasioned the whole epidermis to become loose, and at last it appeared dead. When the woman expired, fatty masses were found in different places under this covering, probably proceeding from the muscles below.

The dried leprous epidermis had a light, but dirty, yellowish-green colour; but during the life of the woman it had been greyish-green. It appeared to be composed of very fine scales laid upon each other, and had a resemblance to shagreen. It was not scurfy, but supple, like the healthy epidermis, though unequally thick.

Being treated exactly as in the preceding analysis, I obtained

Indurated albumen	92 to 93
Mucus, becoming insoluble by evaporation, and Gelatinous mucus, precipitated by nut-galls	6 to 7
Lactic acid, and the above-stated salts (no manganese)	1
Soft fat, which remained dissolved in diluted alcohol, but separated from concentrated alcohol by cooling	$\frac{3}{4}$ to 1

Remarks.

This epidermis did not fuse, like the preceding, which I had collected by degrees from the hard parts of the foot, and it gave by incineration a white ash which did not exceed $\frac{1}{2}$ per cent.

When boiled in water it exhibited the same properties as the healthy epidermis, excepting that it produced an unusual quantity of froth, which appeared to be owing to the mucus.

It was likewise distinguished from the healthy epidermis by a greater proportion of fat, and by the gelatinous mucus which the healthy epidermis does not contain, and which seems in general to be a mark of much local disease. Probably the small scales lying on each other, with which the outer surface was covered, and which gave it a rough appearance, consisted of the gelatinous mucus indurated. It no doubt proceeded from the exhaling vessels at the same time with the perspiration. This gave the patient a very fetid odour. After I had completed my analysis, I learned that M.

Alobert, in his Precis Pratique et Theoretique sur les Maladies de la Peau, Paris, 1810, tom. i., p. 344, Art. VIII.; had published two analyses of herpetic eruptions, and it gave me great pleasure to see that they completely coincided with my own. He discovered an uncombined acid in the scales, which he showed to be phosphoric acid. In the leprous part he found no uncombined acid, but carbonate of lime.

III. *Nails.*

The nails exhibit nearly the same properties as the epidermis. They consist of the same materials, scarcely differing in their proportions. The insoluble portion possesses the characters of indurated albumen.

IV. *Horns of Black Cattle.*

Under this name I reckon the horny sheaths which artists employ in the manufacture of a variety of articles which may be considered as indispensable necessaries of life, as combs, knife-handles, watchmen's horns, boxes, sheaths, &c. They must not be confounded with the bony horns, as those of the hart, &c. which are easily renewed when the animal drops them, and which are composed of quite different materials. These last contain generally much gelatine and earth of bones. The horny sheaths remain attached to animals during the whole of their lives. Sometimes, indeed, they lose them in consequence of disease. Of this the horn of a cow, with which I made my first experiments, furnishes us with an example. The composition of these bodies is quite different from that of true horns, and perhaps it deserves attention that, besides the uncombined acid which I discovered in the epidermis, they contain likewise a peculiar liquid oil. Their composition indicates clearly that they must be considered as indurations and extensions of the epidermis.

(*a*) From 4 to 6 oz. of horn shavings were mixed with 12 oz. of water, and distilled. The distillation was stopped when 5 oz. of the water had come over. The liquid which had come over was milky, and had a very strong smell of horn, while what remained in the retort was free from smell. Even after an interval of some days I could observe no drops of oil floating on the water, but the matter which occasioned the smell of horn had subsided, under the form of a greyish-white cloud. After 8 or 14 days some flocks subsided, but the cloud remained unaltered.

This holds both with the horns of cows and oxen.

(*b*) Fifty grains of the fine shavings of a cow's horn were boiled for an hour in at least 12 oz. of water, and the water was renewed in proportion as it evaporated. By this means it was deprived of four grains of its weight. The concentrated solution did not in the least gelatinize. It reddened litmus paper, and had a very sharp salt taste. Alcohol, and the solutions of barytes, acetate of lead,

oxalic acid, and other acids, caused a precipitate in it, and by repeated evaporation mucus was separated.

(*c*) The undissolved portion did not swell in water, like epidermis and the nails. When heated, it softened, and at last melted. Nitric acid dissolves it completely, and forms much oxalic acid. The alkaline leys likewise dissolve it. When distilled, it gives the same products as the epidermis, without a trace of acid.

(*d*) Alcohol dissolves about one per cent. of fat, and likewise some animal matter (osmazom).

(*e*) When the horn is incinerated it leaves scarcely $\frac{1}{2}$ per cent. of ash, which is white, and composed of the same constituents as the epidermis.

A hundred parts of the horns of black cattle, then, are composed of

Indurated albumen, possessing much of the characters of mucus	90
Gelatinous mucus, with an animal matter thrown down by nutgalls (osmazom ?)	8
Lactic acid Lactate of potash Sulphate, muriate, and phosphate of potash Phosphate of lime Trace of oxide of iron Ammoniacal salt	1
Fat about	1

A peculiar volatile substance, which thickens more rapidly than volatile oil, and has the smell of horn.

Observations.

The presence of an essential oil in the animal kingdom, if we are to judge from the experiments hitherto made, is uncommon. The horny sheaths of animals arrange themselves under that genus of bodies which, like plants, give an essential oil on distillation. Ants, likewise, belong to the same genus. This volatile matter of horns, however, is distinguished from proper oil by not collecting in drops when the liquid containing it is allowed to cool. It is uncommonly volatile, and is separated from the horn by simple digestion in water.

In the horns of black cattle I first detected the uncombined acid, which at times is likewise combined with potash. In all probability no combination of this acid with lime occurs in horns. At least when an acid was poured upon the washed ash, I could perceive no effervescence. Even if such a compound exist, it must be much smaller in quantity than the phosphate of lime. These observations led me to conjecture that the same acid might probably be contained in bones. But experiment did not confirm this conjecture; for in bones quite fresh, and neither boiled nor exposed to a red heat, I observed merely carbonate and phosphate of lime.

V. *Hoof of the Horse.*

The hoof of the horse (either what is called the quick or the sole may be employed) possesses all the characters of horn.

When it is distilled with water, a very fetid liquid is obtained, which contains no perceptible portion of solid matter.

The only other difference which can be perceived is, that the insoluble matter approaches much nearer to caseous albumen than to mucus. When subjected to putrefaction, it assumes exactly the nature of cheese.

The hoof contains no true gelatine. I found no trace of acid in it; and it may be asked whether this substance has been really always wanting to the hoof, or whether it has been abstracted in consequence of the constant moisture to which the hoof is exposed? This may happen the more readily, as Nature has given to the hoof no portion soever of fat, by which it might be defended from the solvent power of the water.

VI. *Horny Excrescence of a Pigeon.*

This example, perhaps the only one known of a similar monstrosity, is to be seen in the Royal Museum. The pigeon, which was full grown, and of the size of a common pigeon, was sent from Nordhausen by Mr. Surgeon-General Görcke. The excrescence had exactly the form of the horn of a he-goat. It grew out of one side of the back, from which the tail feathers had fallen, and had a greater weight than the whole pigeon.

The substance of this horn has a somewhat smutty wax-yellow colour. It is less transparent than horn; and in respect of hardness, is intermediate between wax and horn.

Through the goodness of Professor Rodolphi, I obtained a small portion, which we cut from the hind end in such a manner that the loss would not be perceived by those who examined this extraordinary monster.

By boiling in water, there was dissolved a small portion of gelatinous mucus precipitated by infusion of nutgalls, and likewise traces of alkaline sulphate, muriate, and phosphate.

Alcohol separated a fatty matter, as it did from the horns of black cattle. Cold water produced no effect. When incinerated, it left a very small portion of ashes, which contained an alkali, the above-named salts, phosphate of lime, and gypsum. The undissolved portion of this horny excrescence, which amounted at least to 94 per cent., possessed the properties of insoluble mucus.

ARTICLE III.

On the Chemical Action of Bodies on each other when triturated together. By H. F. Link.*

In a dissertation on Berthollet's theory of affinity (Gehlen's Journal für die Chemie und Physik, vol. iii. p. 240), among other arguments against Berthollet's theory, I stated that bodies are decomposed by merely triturating them together; though, according to Berthollet, this decomposition is only the consequence of a difference in the solubility or volatility of bodies. At that time I paid little or no attention to the water of crystallization, because this water is in the state of a solid body, and cannot act as a medium of solution: and the assertion that all decomposition is the result of easy or difficult solubility, in as far as solubility is concerned, is essential to Berthollet's doctrine of affinity. On that side the theory requires quite other determinations.

It must further be admitted by the supporters of Berthollet's theory that a chemical combination is produced by the trituration of dry bodies together. This combination agrees completely with the theory, and takes place in different proportions. Whether a decomposition takes place depends upon the presence of an easily soluble portion in the compound. Sulphate of potash and muriate of barytes dissolved in water unite together; but the insoluble portion, the sulphate of barytes, separates itself. When I triturate together muriate of barytes and sulphate of copper deprived of its water of crystallization, a combination of all the ingredients of course takes place. But why does alcohol produce no change in it, since it contains muriate of copper, a body easily soluble in that liquid? Similar questions may be put in many other cases, which must at least alter the theory.

But I leave the considerations respecting Berthollet's theory, which a more accurate knowledge of facts have suggested. The experiments on the trituration of bodies with each other ought not, in my opinion, to be entirely neglected, as perhaps some general consequences respecting the chemical action of bodies on each other may be drawn from them.

Muriate of lime and sulphate of copper, both dry, and the latter heated on a metal plate till it fell down in the state of a white powder, remained, after being triturated together, quite white. Absolute alcohol (when I speak of this liquid hereafter, I always mean it in that state) gave the powder a yellow colour. Water rendered it blue. If we triturate crystallized sulphate of copper with muriate of lime, the powder has a yellow colour. Muriate of

* Translated from Schweigger's Journal, vol. xiv. p. 193, October, 1815.

barytes and anhydrous sulphate of copper triturated together remain white. Alcohol does not alter the colour. Water gives it a blue colour. I must here put the reader in mind that muriate of copper with little water is yellow; but, when united with much water, blue.

In these experiments the difference between muriate of lime and muriate of barytes consists in this, that the former is soluble in alcohol, while the latter is insoluble in that liquid. Solution, then, is necessary to chemical action. The water of crystallization of sulphate of copper acts entirely as uncombined water, and the chemical action of the water is not the consequence of its liquidity, but is peculiar to it.

Acetate of lead in crystals, and anhydrous sulphate of copper, when triturated together, remain white, and do not alter one another. But when acetate of lead and crystals of sulphate of copper are triturated together, the mixture assumes immediately a fine green colour. Alcohol gives to the white powder a shade of green, and water renders it much more green. As acetate of lead in crystals decomposes crystallized sulphate of copper, but not the anhydrous sulphate, it is probable that it contains no water of crystallization, at least none in a state capable of acting. Acetate of lead is soluble in alcohol, though only in small quantity.

Acetate of lead and burnt alum just heated, and scarcely cooled, being triturated together, produced no alteration on each other, and afterwards liquefied very slowly when left in an open vessel. But when the burnt alum had been kept for some time in a vessel not very well stopped, it very soon became liquid when triturated with acetate of lead. The moisture of the atmosphere, therefore, does not act directly as a medium of decomposition, but only when it has been absorbed by a solid body, and deprived of its fluidity.

One part of muriate of lime and two parts of sulphate of potash, being triturated together, became at first somewhat moist, but gradually dried again when left exposed to the air. The taste at first was that of muriate of lime, but it became gradually weaker, and at last assumed a salt taste.

Prussiate of potash and sulphate of copper, both heated till the water of crystallization was evaporated, when triturated together, remained white. Alcohol did not alter the colour. Water rendered it reddish-brown. If we take crystallized sulphate of copper instead of anhydrous, the mixture on trituration assumes a reddish colour. The same thing takes place when anhydrous sulphate of copper is triturated with crystallized prussiate of potash.

Anhydrous acetate of copper, being triturated with anhydrous prussiate of potash, produced a green colour. Alcohol did not alter the colour; but when left for some days in an open vessel, the colour became lavender-blue. Water rendered the colour reddish-brown. I may here put the reader in mind that cold alcohol dissolves little or no acetate of copper.

Carbonate of ammonia and anhydrous acetate of copper, when

triturated together, remained whitish-green. Alcohol at first produced no alteration on this mixture; but by degrees the colour became blue. Water poured upon the mixture occasioned effervescence, and the colour became blue. Both these salts are insoluble in cold alcohol.

Quick-lime heated a second time, being triturated with calomel, the mixture remained white. Alcohol did not alter the colour. Water rendered it blackish. Quick-lime and corrosive sublimate triturated together remained white, but became brownish-red when left for some time exposed to the atmosphere. Not only water, but alcohol also, produced this change on the mixture. It is well known that calomel is insoluble, and corrosive sublimate soluble, in alcohol.

Effloresced carbonate of soda heated, and anhydrous sulphate of copper, being triturated together, remained white. Water gave the mixture a green colour. Alcohol did not alter it. In the air it became green. The same soda triturated with calomel remained white. Water rendered the mixture brownish, and then blackish. Alcohol produced no alteration. The same soda triturated with corrosive sublimate remained white; but water and alcohol rendered the mixture brownish-red.

Prussiate of potash and green sulphate of iron, both anhydrous, being triturated together, remained white. Alcohol gave the mixture a grey, and water a blackish, colour. The green sulphate of iron was not quite free from persulphate, which is soluble in alcohol.

Powdered nutgalls heated, and triturated with anhydrous protosulphate of iron, occasioned no change of colour. Alcohol did not alter the colour, but water rendered it black.

Litmus, heated till it was quite dry, and then triturated with succinic acid, underwent no change of colour. That water poured upon the mixture should give it a red colour, might have been expected; but alcohol produced no alteration in the colour. But when the alcohol was evaporated, the colour became red; and this alteration took place the sooner the more fully the mixture was exposed to the air. Alcohol, then, facilitates the absorption of moisture from the atmosphere. The reddening of litmus tincture proceeds entirely from water, for succinic acid is soluble in alcohol. The same phenomena take place with benzoic acid.

I triturated together sulphur and phosphorus. After some time, the two bodies united into a yellowish liquid. Mr. Schaub first made this observation (Scherer's Allg. Jour. der Chemie, vol. viii. p. 217). He thought that an oxidation was produced by this process. But the liquidity is not owing to this cause; for the substance again becomes solid when left to itself; and the same thing takes place when cold water is poured upon it. Hence the liquidity of this very easily liquified compound is occasioned by the heat evolved during the trituration. But the compound is very easily oxidized, as water left upon it reddens litmus tincture. When

heated under water, phosphureted hydrogen gas is evolved, which occasions an explosion. Alcohol acts upon this compound, and forms a solution, having a very fetid odour. It is known that a moderate heat produces a combination between sulphur and phosphorus.

From these experiments the following consequences may be drawn:—

1. The trituration of anhydrous bodies produces no chemical action.

2. But it takes place when one or both bodies is soluble in the liquid poured upon them.

3. It takes place equally when one of the bodies is soluble, the other insoluble, in the liquid poured on the mixture.

4. The water of crystallization acts as free water. But the moisture of the atmosphere acts only when it is absorbed by one of the triturated bodies.

5. The consequence of the decomposition has no effect on its success. It is the same thing whether the body produced by the decomposition be soluble or not.

What is called disposing affinity might, in consequence of this last circumstance, be rejected.

The reddening of tincture of litmus is an action of acids depending entirely on the presence of water.

Finally, there are chemical compounds which are formed entirely in consequence of the heat evolved by the trituration; for example, the compound of sulphur and phosphorus.

Article IV.

A Comparison of the Old and New Theories respecting the Nature of Oxymuriatic Acid, to enable us to judge which of the two deserves the Preference. By Jacob Berzelius, M.D. Professor of Medicine and Pharmacy, and Fellow of the Royal Academy of Sciences at Stockholm.

(*Continued from* p. 280.)

7. *Chlorine combines with Azote. The Compound is an oily-form Liquid, which explodes violently at the Heat of boiling Water, because its Constituents separate.*

To be able to judge rightly of these facts, we must make a short digression on the *explosions of chemical compounds,* and on the *appearance of fire* which takes place during these explosions.

Of the hypotheses which have been formed to account for the increase of temperature during chemical combinations, which often proceeds so far that fire actually appears, that one only agrees with all the phenomena, and accords with the whole of science, which ascribes the heat and the fire produced in chemical combinations to

the same cause as in electrical discharges. I need not here state the great number of facts, which have obliged us to give up the old opinions and adopt the new, as they must be known to all those who have followed the progress of the science.

Fire, according to this theory, is produced by the neutralizing of the opposite electro-chemical states of the bodies entering into combination; and it is put beyond all doubt by experiments that the more opposite the state of electricity between two bodies is, the more intense is the appearance of fire when they combine. When, therefore, two bodies, A and B, are united, and a third, C, is presented capable of neutralizing the electro-chemical state of A more completely than B can; then B will be displaced by C with an increase of temperature, because the new and more complete electro-chemical neutralization always produces an increase of temperature. Thus gold and silver unite very weakly to oxygen. Hence it may be conjectured that when they combine with it only a very small increase of temperature is produced. When the oxides of these metals are reduced by potassium or hydrogen, or iron or charcoal, fire always makes its appearance.

Observations show us that bodies which have no great opposition in their electro-chemical properties, those, namely, which have only weak affinity for each other, are capable of combining only in very low temperatures, and are again decomposed in higher temperatures. On the other hand, it is very common to find bodies which have a stronger affinity for each other combining only in high temperatures. In low temperatures bodies usually obey weak affinities, and the compounds produced are decomposed in higher temperatures with greater or lesser rapidity, because then stronger affinities act; and when there is a very great difference in the degrees of the uniting and decomposing affinities, the decomposition is attended with the appearance of fire, and with an explosion.

We see from this why *fulminating silver*, *fulminating gold*, &c. exist in a given temperature, and in another temperature are de composed of themselves with an explosion, and the appearance of fire. The affinities which give existence to fulminating silver are those of hydrogen to azote,* of silver to oxygen, and of ammonia to oxide of silver. Each of these is weak, and is destroyed in a higher temperature. Hence it follows that fulminating silver must be destroyed at a high temperature. But it will be asked how this decomposition takes place in a low temperature, why fire is produced, and whence proceeds the dreadful violence of the decomposition? All these proceed from the burning of the hydrogen at the expense of the oxygen in the oxide of silver, or from the more complete electro-chemical neutralization of the oxygen and hydrogen in water than in fulminating silver.

We learn from satisfactory experiments that, when two bodies of opposite electro-chemical properties act upon each other, an *elec-*

* Not to puzzle the reader with too many little known theoretical views, I here set aside the probably accurate opinion that azote is a suboxide of *nitricum*.

trical polarization takes place between them, which increases as their temperature approaches nearer to that in which their peculiar affinities can act, when the polarization vanishes at the same time that their union is completed, with the appearance of fire. Such a polarization, therefore, must take place between the oxygen and hydrogen in fulminating silver; and it must be the greater the less of their original electro-chemical properties is neutralized in the other compound. Experience likewise teaches us that the affinities are more active in solid and liquid bodies at low temperatures than in gases; and that they act in condensed gases more readily than in gases of the usual density.*

From all these observations it may be concluded that in fulminating silver the polarization is nearly at its maximum (that is, at the discharging or uniting point), so that a very small cause will bring it to that state. Hence we see the reason why a small touch will make fulminating silver explode, either from the elevation of temperature, or from the power which friction has to excite electricity. But whence comes the extraordinary rapidity with which the decomposition takes place? Can it be explained by the rapid communication of the high temperature, or the combustion? Experience teaches us that the propagation of heat is not particularly rapid, and that it is very far from instantaneous, even in liquid bodies. The rapidity increases with the increase of temperature, but it always requires time. By the mere propagation of heat gradually evolved by combustion, it is impossible to explain the immeasurable rapidity of an explosion by which a cannon is burst before the ball has time to be put in motion. On the other hand, experience shows us that the propagation of electricity may be considered as instantaneous. In the exploding compound there are two or three bodies almost at a maximum of electrical excitement. Hence we may conceive how this excitement discharges itself at once in an immeasurably small period; and by the combination of substances which at that temperature are gaseous, the dreadful phenomenon which we call explosion may be explained.

The electro-chemical theory, then, explains all the appearances of an explosion in a satisfactory manner, and corresponding with all the rest of chemical science. It shows us that an explosion cannot take place unless when a compound (or a very complete mechanical mixture) can arrange its constituents in other proportions, by means of which their opposite electro-chemical properties can be much more completely neutralized than before.

But now the question occurs: as in each chemical combination an increase of temperature takes place, which often amounts to fire, does the same take place in opposite circumstances, or during chemical decomposition? We have no theoretical ground to deny this. We are ignorant whether fire consists in electrical discharges;

* For example, fulminating air is not set on fire by a red-hot iron, but it takes fire of itself when it is strongly compressed.

and as long as we do not know this, it is impossible for us to say whether it can take place or not by a separation of electricities. But whether an increase of temperature, or the appearance of fire, in reality takes place in such cases, we may determine by experiment. The question, then, comes to this: do we know any example of two combined bodies, whose simplicity is undisputed, that separate from each other with an increase of temperature produced by the separation itself, and which assumes a state of complete disunion? For my part, I am acquainted with no such example; for that in the present case neither euchlorine, nor chloride of azote, nor iodide of azote, can be admitted, is obvious.

None of the easily reducible metallic oxides gives the least sensible increase of temperature when it is reduced by means of heat, and the reduction ceases as soon as the temperature is lowered; which would not be the case if the process of separation occasioned an increase of heat, which, at least in some cases, would be able to complete the process without the aid of external warmth. If such an evolution took place, the red oxide of mercury, when heated to the decomposing point, must explode; for example, when it is thrown into a platinum crucible at a white heat. But, though both the oxygen and mercury at that temperature are gaseous, the oxide is only slowly reduced as it comes in contact with the crucible. Hence we may say that in this case, as well as in that of boiling water, heat becomes latent; and that, of consequence, in chemical decompositions, heat is rather absorbed than evolved.

When the affinity between two bodies is destroyed by an increase of temperature, we cannot suppose that this takes place in consequence of an annihilation of the affinity, so that the united bodies are instantaneously separated from each other, just as a body hanging by a string falls to the ground when we cut the string. The actions of the affinity and of the temperature are to be considered as two powers acting in opposition to each other, in consequence of which the affinity, when overcome, is instantaneously prevented from acting sensibly. We see from this that an increase of temperature can produce no instantaneous decomposition in a large mass, especially when we take into view the slowness of the propagation of heat. But as it is certain that each electrical neutralization and chemical combination is accompanied by an increase of temperature, it is obvious that, if electro-chemical decomposition were accompanied by the same increase, this would not be confined to a few rare examples, but would be a necessary and constant concomitant of every decomposition. But as it has been observed only in a few rare cases, we may conclude it follows with tolerable certainty that an increase of temperature is not produced by decomposition.

From what has been said, it follows that an explosion, which cannot take place without an evident increase of temperature produced by itself, cannot well accompany the separation of two elementary bodies, which by the separation are reduced each to a state

of simplicity. The phenomenon shows clearly that either all, or at least one, of the separating bodies, is compound; and that during the explosion the constituents are differently arranged.

* * * * * *

I now return to the *chloride of azote.* The new doctrine considers this body as a compound of chlorine and azote. When the temperature is slightly elevated, both the elements separate with an explosion, and the production of fire. The new doctrine acknowledges the difficulty of explaining the explosion, but denies that on this account any consequence can be drawn against its accuracy.

The old doctrine considers this peculiar substance as a compound of muriatic acid and nitrous acid (or nitric acid) both free from water, because these acids are obtained when the exploding compound is exposed to the action of water in a close vessel. As oxymuriatic acid at a high temperature retains oxygen much more powerfully than charcoal does, it is obvious that in a high temperature muriatic acid must decompose nitrous acid, and this new combination must be accompanied by fire. The red-hot oxymuriatic acid gas and azotic gas occasion the explosion.

When this compound is touched by a combustible body containing hydrogen, the hydrogen unites with the oxygen of the nitrous acid, and forms water, which unites with the muriatic acid. But as this process occasions an increase of temperature beyond what the compound can bear, the whole explodes at the instant of contact.

To put it in the reader's power to judge more accurately of this view, I will endeavour to give an answer to the following questions: How can we conceive this body to exist in water, if it be composed of anhydrous muriatic acid and nitrous acid, while yet muriatic acid in a high temperature has an infinitely greater affinity for oxygen than azote has? How is an anhydrous acid different from an hydrous acid? What is a double acid? And do we know any such, besides those produced by chlorine, fluorine, and iodine.

When oxymuriatic acid gas acts at a low temperature on an ammoniacal salt dissolved in water, the oxymuriatic acid by the hydrogen of the ammonia is reduced to the state of hydrous muriatic acid; and as the azote, at the instant of its disengagement, is in contact with oxymuriatic acid still undecomposed, it is enabled, in consequence of the affinity of azote for oxygen, together with that of nitrous acid for muriatic acid, to overcome the simple affinity of muriatic acid for oxygen, it decomposes the oxymuriatic acid, and, both acids uniting together, form an insoluble compound, which falls to the bottom. But how it comes to pass that in this new arrangement of the constituents which takes place in a low temperature, the oxygen still retains the greatest part of its original electro-chemical polarization towards the muriatic acid, though it remains in combination with the azote, and exists in the compound as a constituent of the nitrous acid (without which the explosion could not take place) cannot in the present state of our electro-chemical knowledge be fully explained. But this is no objection to

the accuracy of the old doctrine, as we know several other examples of the same kind, the explanation of which must remain till our knowledge be further advanced. Many other bodies susceptible of an higher degree of oxidation mutually decompose each other at different temperatures, as happens in the present case with azote and muriatic acid. When, for example, a cold solution of proto-sulphate of iron is poured into sulphate of silver, the silver is precipitated by the oxide in the metallic state, and the ferruginous salt is converted into persulphate of iron. If the mixture be now boiled, the silver again takes oxygen from the iron, and we obtain a solution of proto-sulphate of iron and sulphate of silver. This phenomenon is still more striking when powder of silver is boiled in a somewhat concentrated solution of persulphate of iron. How far soever our knowledge shall advance, we must expect still to meet with inexplicable facts.

It is now asked, How can an anhydrous acid be accurately distinguished from a hydrous acid? And how the anhydrous state contributes something to elucidate the nature of the exploding compound which differs from a compound of common acids? Though none of the anhydrous compounds of muriatic acid with phosphoric acid, phosphorous acid, &c. can be produced as an example, yet whoever will cast his eye over the whole class of these compounds will find many instances.

It has been ascertained, both by my own experiments and those of other chemists, that many of the more powerful acids, which the older chemists considered as pure, are combined with water, which serves them as a basis, without, however (as is the case with most other bases), diminishing their acid properties. Several of these acids are of such a nature that chemistry knows no method of exhibiting them in an anhydrous state. Others of them may be brought to a state of purity, and then show very striking properties, which deserve more attention from chemists than has hitherto been bestowed on them.

If we cast an eye upon phosphoric and boracic acids rendered free from water by heat, and upon dry carbonic acid gas, we perceive at once that in this state they have lost a good deal of their acid properties, which they only recover by the addition of water. When glassy phosphoric or boracic acid in powder is brought in contact with ammoniacal gas over mercury, no absorption of the gas takes place, and no ammoniacal salt is formed. But if a moist paper be introduced into the jar, and left on the surface of the mercury, the formation of phosphate or borate of ammonia instantly begins, and continues till the paper is thoroughly dry, and all the water has entered into combination with the salt. From this it seems to follow that no simple ammoniacal salt can exist without combined water. But as sub-ammoniacal salts exist without water, and as boracic acid is capable of forming sub-salts, a sub-borate of ammonia, in such a case, might be supposed capable of being formed. If well-burnt lime, free from water, be brought

in contact with dry carbonic acid gas, the gas is not absorbed, or the absorption is scarcely perceptible. But allow aqueous vapour to enter, and the absorption is completed in a few minutes, though water does not constitute one of the constituents of carbonate of lime. Whoever has studied the experiments of Mrs. Fulhame is acquainted with other instances of the same kind. Water, therefore, produces an alteration in most oxidized substances, whereby they enter more easily at low temperatures into combination with other oxides. We are not yet well acquainted with the nature of this action of water, and of all the theoretical explanations of it that have been proposed, there is not one worthy of adoption.

It is known that in the distillation of green vitriol a smoking, liquid, crystallizable matter, the nature of which was long unknown, follows the sulphuric acid. Mr. Vogel, of Bareuth, found that this peculiar body formed, with lime, gypsum; with barytes, sulphate of barytes; with soda, Glauber's salt; and with water, common sulphuric acid. Owing to the great difference of the physical properties of this body from those of common sulphuric acid, he did not draw the very natural conclusion that, as it forms common sulphuric acid with water, it must be an anhydrous sulphuric acid. He found, likewise, that when sulphur was heated with this body, it formed at least two combinations with it, neither of which was sulphurous acid. Mr. Vogel appears, therefore, to have discovered new oxides of sulphur, by reducing anhydrous sulphuric acid by means of sulphur, which are very highly deserving of the attention of chemists.

From what has been said, it is evident that anhydrous acids (not saturated with a base) may be accurately distinguished by means of their physical characters from the same acids combined with water—a difference which may very well be compared to that between a combustible radical and its oxide, or perhaps any other of its compounds.

The compounds formed by two or more acids are well entitled to the attention of chemists, especially as this class of bodies has not been long known, and therefore has not been much examined. I cannot here notice the compound acids into which muriatic acid and fluoric acid enter as ingredients, but must confine myself to the compound acids, which are acknowledged by both doctrines.

If highly concentrated sulphuric acid be brought in a small vessel in contact with nitrous gas over mercury, the gas is not absorbed by the acid, a proof that in this way no sulphate of nitrous gas is formed. If we introduce a little oxygen gas, nitrous acid is formed, which is absorbed by the sulphuric acid, and forms with it a small crystallized compound. If we continue to add oxygen gas in small quantities till no more nitrous acid is absorbed by the sulphuric acid, the whole is converted into a magma full of plumose crystals. The crystals may be separated from the liquid by throwing the whole into a funnel filled two-thirds with pounded glass, and covering the mouth of the funnel with a glass plate, to keep out the

moisture. The crystallized body obtained by this means is a chemical compound of sulphuric acid and nitrous acid, in which the latter contains one-third of the oxygen contained in the former ($= N O^4 + 4 S O^3$). It seems, likewise, to contain some water.* When this compound acid is gently heated, it melts like fat, but crystallizes on cooling. When mixed with a little water, it is decomposed, and converted into common sulphuric acid and hydrous nitrous acid, and this last liquid, according to the proportion of water, is yellow, green, or blue. When so much water is added as to render it colourless, the nitrous acid is completely decomposed into nitric acid and nitrous gas, just as is the case with common red nitric acid. When the compound acid is distilled in a glass retort, the nitrous acid is converted (partly by absorbing oxygen, partly by giving out nitrous gas) into nitric acid, and we obtain a compound of sulphuric and nitric acids, which cannot be decomposed by distillation (unless they be previously diluted with water). This compound acid is heavier than common sulphuric acid, and may be obtained of the specific gravity 1·94 or 1·96. It does not crystallize when cooled down, and dissolves metals with the evolution of nitrous gas.

We have here an example of two compound acids of *sulphuric acid*, the one with *nitrous acid*, the other with *nitric acid*. Hence we see that other compound acids exist besides the disputed ones; and have ground to conclude that similar compounds of the other acids, namely, muriatic acid, phosphoric acid, fluoric acid, iodine with nitrous and nitric acids, must exist, and only require investigation to be discovered. We perceive, therefore, that the difficulty of accounting for the explosion of chloride of azote is not the only reason for considering this body as a compound of muriatic acid with nitrous or nitric acids.

We are not entitled to demand of the old doctrine an explanation why the acids combine together in the middle, or at least on the surface, of water, and then are brought by the water slowly, and with difficulty, to the state of hydrous acids, though they have a stronger affinity for water than for each other. Neither will we require of the new doctrine to explain why, although the affinity of chlorine to hydrogen, and of azote to oxygen, be greater than that of oxygen to hydrogen (so that chloride of azote gradually decomposes water at the common temperature, and forms muriatic acid and nitric acid), yet these acids are not formed at once, as chloride of azote may be obtained in the midst of, or on the surface of, water. We see that the difficulty of explaining these facts is the same, whatever doctrine we embrace.

Gay-Lussac has very properly stated the difficulty of an explosion from mere decomposition, and asks (Gilbert's Annalen, vol. xlix.

* A very interesting method of obtaining this compound acid, but not instructive with respect to its nature, will be found in Davy's Elem. of Chem. Phil. p. 276.

p. 31), whether the appearance of fire be owing to the shock which the gases, instantaneously set at liberty, give to the surrounding air, as we know that the compression of air produces heat, which may very well amount to redness. But this does not explain the principal phenomenon of the explosion, namely, the extraordinary energy with which the gaseous bodies are set at liberty: and there is another appearance which puts the insufficiency of that explanation beyond doubt. If a portion of the compound be exploded in a vessel containing air, the air is distinctly expended during the explosion, and again condenses, which could not take place if the fire of the explosion were the consequence of the compression of the air; for in that case the air, during the explosion, must occupy a smaller space than afterwards, when it has time to expand, and to absorb the heat which it has lost.* From what has here been said, it follows that the explanation of Gay-Lussac is not accurate, and that the elevation of temperature must be owing to the chemical process which precedes the explosion, and must proceed from the same cause as that of fulminating silver, fulminating gold, &c.

We have now to explain what takes place in the explosion of euchlorine. According to the old opinion, this gas is the second super-oxide of the basis of muriatic acid in which that basis is com-

* Gay-Lussac inquires whether in this situation we may not have recourse to electricity, as we know so many decompositions produced by that agent. From this question, as well as from what he says in the Appendix to his Treatise on the Neutrality of Compounds, we may conclude that hitherto he has not paid much attention to electro-chemical studies. As this, on the one side, is rather surprising, so on the other side it is very encouraging, that even his chemical studies obliged him to have recourse to this universally distributed agent, as the new science has reason to expect much from the uncommon talents of this distinguished man. Gay-Lussac, among other things, mentions as a fact still unexplained, that when a saturated solution of nitrate of ammonia is mixed with its own bulk of water, the bulk is diminished, and at the same time cold produced. I shall make a few observations on this subject. In my Treatise on the Water of Crystallization I have already endeavoured to draw the attention of chemists to the difference between solution in water and combining chemically with water. Carbonate of magnesia combines chemically with $\frac{1}{3}$ of its weight of water, but is insoluble in that liquid. Nitrate of potash contains no chemically combined water, yet it is soluble in that liquid. When a body unites chemically with water, heat is produced; when it dissolves in water, cold is evolved. If muriate of lime previously heated to redness be moistened with water, heat is first produced, because the water of crystallization combines; but when more water is added, cold takes place. The principal cause of the heat is not so much the condensation of water as its chemical combination, that is, the opposite electro-chemical neutralization of the water and the salt. The cause of the cold by the solution, on the contrary, is the increase of the volume, and the easy solubility of the salt, which must now spread itself through the whole mass of the water. The sum of the bulk of both is diminished, because the water, while it receives the salt into its pores, and is diluted, contracts itself through the action of the salt. Now as the salt requires more heat, in consequence of the proportionally much greater separation of its molecules, than the water gives out in consequence of the approach of its particles, cold ensues. (In the opposite case heat would be produced.) The greater the proportion of water compared to that of the salt, the more heat is absorbed, though the mixture does not lose as much temperature as when the quantity of liquid and the mass to be cooled is small. Gay-Lussac's difficulty is explained by this, that the molecules of salt double their distance, while those of the water are only condensed a very little.

bined with twice as much oxygen as in muriatic acid. This gas explodes in a heat of between 86° and 104°. Fire is produced, and the gas occupies $1\frac{1}{2}$ times the volume which it formerly occupied, being resolved into $\frac{1}{3}$ oxygen and $\frac{2}{3}$ oxymuriatic acid gas. I explain these appearances in the following manner:—Muriatic acid combines at a certain low temperature less intimately with two atoms of oxygen, with which it makes its escape as a gas from the liquid. In this compound the electro-chemical polarization of the oxygen is less completely neutralized than in the compound of muriatic acid with half as much oxygen in oxymuriatic acid gas. When the temperature is elevated, the muriatic acid cannot retain the whole of the oxygen; it therefore enters into a more intimate combination with one half of it, and undergoes a combustion into oxymuriatic acid, fire being evolved in consequence of the more complete electro-chemical neutralization. The other half of the oxygen is set at liberty. The separation of this portion has no other effect upon the explosion than that of increasing the volume of the gaseous mass, and consequently the energy of the explosion.

This explanation appears at first sight liable to two objections. The first is, that the muriatic acid, which was here in the state of a hydrous acid, should separate from the water to unite itself in an elastic state with two proportions of oxygen less intimately combined. But it is a very common appearance when a gaseous or insoluble body is formed by the play of affinities for the liquid body to remain while the gaseous body disappears, or is precipitated from the liquid, though its formation be owing to a weaker affinity. The muriatic acid here leaves the water, which should have retained it in order to combine with oxygen, and form euchlorine. In the same way concentrated phosphoric or arsenic acid separates the much stronger sulphuric acid from its bases whenever the mixture reaches the temperature at which anhydrous sulphuric acid becomes gaseous. Chemistry can exhibit many such examples. The explanation of them belongs to a department of the doctrine of heat still unexplained, and to its relation to both the electricities. The explanation furnished here by the old doctrine affords no anomaly different from what takes place in other bodies.

The second apparent objection is, that the muriatic acid combines less intimately with two portions of oxygen than it does with the one portion with which it constitutes oxymuriatic acid. But it is clear that, provided the same difference in the intimacy of combination takes place between other bodies in different states, this explanation will furnish no objection or improbability. I will now show that such a difference in the intimacy of compounds is a very general appearance, which takes place not only between simple bodies, but likewise between compounds, to which hitherto but little attention has been paid.

When, in the year 1811, I was occupied with examining the combinations of antimony, I discovered accidentally that several metalline antimoniates, when they begin to grow red-hot, exhibit

a sudden appearance of fire, and then the temperature again sinks to that of the surrounding combustibles. I made numerous experiments to elucidate the nature of this appearance, and ascertained that the weight of the salt was not altered, and that the appearance took place without the presence of oxygen. Before the appearance of fire, these salts are very easily decomposed, but afterwards they are neither attacked by acids nor alkaline leys—a proof that their constituents are now held together by a stronger affinity, or that they are more intimately combined. Their opposite electro-chemical polarity must, therefore, be more completely neutralized, by which these bodies are brought to a state of chemical indifference. The cause of the appearance of the fire, therefore, is that in a higher temperature a more intimate chemical combination, or a stronger electro-chemical discharge must take place between the bodies. Hence it follows that between the same bodies different degrees of intimacy in point of combination may subsist. From my dissertation it will be seen that I even at that time foresaw that these facts would furnish a key to explain the explosion of euchlorine.

Some time after, during my short stay in London, I mentioned this appearance to Wollaston and Davy. The former observed that, to his great surprise, he had seen something similar in gadolinite.* Davy had observed the same appearance of fire on heating the hydrate of zirconia, which he ascribed to a contraction of the earth at the instant when the water separated. Since that time I have observed these appearances in many other bodies; as, for example, in green oxide of chromium, oxide of tantalum, and oxide of rhodium. I shall take oxide of chromium as an example.

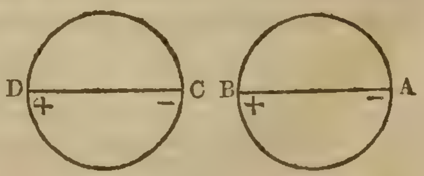
Upon pulverized chromate of lead let a mixture of concentrated muriatic acid and alcohol be poured. Heat is produced, and ether, muriate of lead, and muriate of chromium, formed. Let the solution be mixed with more alcohol, in order to separate all the lead salts. Then distil off the alcohol, dilute the solution again with water, and precipitate all the oxide of chromium with caustic ammonia, adding a slight excess of alkali. Let the greenish-grey precipitate, which is a hydrated oxide of chromium, be separated, dried, and heated, in a crucible or small retort till it be slightly red-hot. Water is given out, and the oxide becomes greyish-black, or almost black. Let it now be taken from the fire, weighed, and placed in a strong heat. As soon as it becomes red-hot we shall perceive it all on a sudden become intensely ignited, and this ignition disappears almost as suddenly. Oxide of chromium, by being thus treated, loses no weight. Its colour is pale green; and instead of its easy solubility in the state of hydrate, it has become, when

* The appearance of fire which gadolinite displays is very lively. The variety with a glassy fracture answers better than the splintery variety. It is to be heated before the blow-pipe, so that the whole piece becomes equally hot. At a red heat it catches fire. The colour becomes greenish-grey, and the solubility in acids is destroyed. Two small pieces of gadolinite, one of which had been heated to redness, were put in aqua regia; the first was dissolved in a few hours; the second was not attacked in two months.

deprived of water, difficultly soluble, and after ignition completely insoluble. In this case a new combustion takes place between the oxygen and chromium already combined; that is to say, a new electro-chemical discharge, by which the elements not only combine more intimately, but the oxygen has lost its former properties, or its former electro-chemical polarization has been exchanged for a complete electro-chemical indifference.* It is clear that if the oxide of chromium at this temperature were gaseous, the production of fire would cause it to explode, without the ingredients undergoing any new combination with another body (perhaps even a separation), and without the oxide of chromium ceasing to be the same compound, and in the same proportions as before. If we could obtain chromic acid free from water, and in a separate state, probably when exposed to a higher temperature it would exhibit the same appearance of fire and separation of oxygen as takes place with euchlorine in the same circumstances.

Edmund Davy found that when a neutral solution of platinum was precipitated by hydro-sulphuret of potash, and the precipitate dried in air deprived of oxygen, a black compound of sulphur was obtained, which, when heated out of the contact of air, gave out sulphur, and some sulphureted hydrogen gas, while a combustion similar to that in the formation of the metallic sulphurets appeared, and common sulphuret of platinum remained behind. In this case the very same phenomenon is observable as in euchlorine. The platinum combines at a low temperature loosely with a greater proportion of sulphur than it can retain in a higher temperature. When the compound is heated there is produced fire, because the platinum combines more intimately with a portion of the sulphur, and another portion which cannot be retained at that temperature is disengaged.

I have found that when we heat the oxide of rhodium obtained from the soda-muriate of rhodium, water first comes over; and on increasing the temperature, combustion takes place, oxygen gas is suddenly disengaged, and a sub-oxide of rhodium remains behind. Here again we have the same appearance as with euchlorine. Rhodium has this in common with the base of muriatic acid that its first and third oxides are salifiable bases, while the second possesses the characters of a super-oxide, giving out oxymuriatic acid when digested with muriatic acid, and forming salts with no acid, but in some measure combining itself with the bases. The peroxide, on the contrary, is a well marked salifiable base; but its oxygen is less intimately combined. It cannot be obtained from



* If we adopt the theory, and place the electro-chemical properties of bodies in the electro-chemical polarity of their smallest parts, the first combination may be produced by the discharge of the two poles, B, C. The compound is still polar by the electricity of A and D, that is, it possesses those properties which it loses by the discharge of A, D, because the body then becomes indifferent.

from the inferior oxides, as these have the oxygen more intimately combined; but is procured only at lower temperatures, and under favourable circumstances by the abstraction of the excess of rhodium.* The base of muriatic acid gives, in the same way, first an acid, then two super-oxides, then an acid, which can be procured only under favourable circumstances, and by no means directly.

We have now, I conceive, fully shown that different degrees of intimacy subsist between oxygen and the same combustible basis, frequently between the same proportions. The combustible body in a low temperature often unites less intimately with a greater number of atoms of oxygen, and then at a higher temperature enters into a closer combination with a smaller number of atoms, by which fire is produced, and the excess of oxygen is set at liberty. We have seen that this difference in the intimacy of the combination takes place, not only between combustible bodies and oxygen, but likewise between other bodies, both simple and compound, as is evident from the experiments on the production of sulphuret of platinum by the moist way, on the metalline antimoniates and the siliciate of yttria. This more intimate union, then, is a general appearance, and it cannot be alleged that it has been contrived merely for the purpose of accounting for the explosion of euchlorine. It is evident, therefore, that the explanation furnished by the old doctrine agrees fully with every other department of chemical science.

(*To be continued.*)

Article V.

Trigonometrical Survey of the Wide Mouth Shoal, or Royal Sovereign's Shoal, near Beachy Head, in the English Channel. By Col. M. Beaufoy.

[For the Diagram see Plate L. Fig. *b*, *c*.]

Observations made with a Hadley's sextant for determining the situation of the Wide Mouth Shoal, or the place on which some years past the Royal Sovereign man of war grounded, and was nearly lost, and which shoal is in a book of pilotage denied to exist.

Triangle, I, B, H.

Observed angles..	H, Q, B, 25° 57′ 20″	Given ..	I, B,	71,901 feet.
	B, Q, I, 62 38 25		H, B,	48,290
	I, Q, S, 21 26 20		I, B, H,	55° 59′ 58″

* See my treatise On the Cause of Chemical Proportions, *Annals of Philosophy*, vol. iii. p. 255.

	Feet.	Nautic Miles.
Q, H, shoal from Bexhill Church	38,903 or	6·34
Q, B, shoal from Brightling Mill	80,167 ..	13·10
Q, I, shoal from Willington Mill	46,842 ..	7·65
Q, S, shoal from Beachy Head Flag Staff ...	40,466 ..	6·61

Let M, N, be the meridian of Brightling Mill;
B, H, the distance equal to 48,290 feet;
T, H, the departure equal to 23,552 feet, and
gives the angle N, B, H; the bearing 29° 11′ 28″ S. E.
N, B, I, ... 26 48 24 S.W.
N, B, S, ... 19 21 46 S.W.

N, B, H,	29°	11′	28″	N, B, I,	26°	48′	24″	S, B, H,	48°	33′	05″
Q, B, H,	20	38	48	Q, B, I,	35	21	10	Q, B, H,	20	38	48
N, B, Q,	8	32	40	N, B, Q,	8	32	46	S, B, Q,	27	54	17
					8	32	40	N, B, S,	19	21	46
					8	32	31	N, B, Q,	8	32	31
				3)	25	37	57				
				Mean	8	32	39				

B, Q, R, = Q, B, N, = 8° 32′ 49″

H, Q, B,	25°	57′	20″	B, Q, I,	62°	38′	35″	B, Q, S,	84°	4′	55″
B, Q, R,	8	32	39	B, Q, R,	8	32	39	B, Q, R,	8	32	39
	17	24	41		71	11	14		92	37	35

Bexhill Church bears from the shoal 17° 24′ 41″ N. E., distant 38,903 feet, or 6·34 nautic miles.

Willington Mill 71° 11′ 14″ N. W., distant 46,482 feet, or 7·65 nautic miles.

Beachy Head Flag Staff 92° 37′ 35″ W. of the N., distant 40,466 feet, or 6·61 nautic miles.

Brightling Mill 8° 32′ 39″ N.W. distant 80,167 feet, or 13·10 nautic miles.

60,851 fathoms is a degree of latitude.

Q, H,	6,400	Nautical miles and fathoms.
Q, B,	13,179	
Q, I,	7,709	
Q, S,	6,660	

The bearings are the true points. The shoal is of a circular form, about 500 feet in diameter, and has 13 feet water at low spring tides. It is also the outermost shoal, the Horse of Willington being much within it.

Marks for finding the Shoal.—Murray's Tent on with the East

Knoll called Tillum, and the Grove near Hollywell on with the Chalk Pit and three Bergs.

Marks for avoiding the Shoal.—In coming up the Channel, and when round Beachy Head, where is a spot called Greenland, keep this spot open with the Bluff Head, and steer E. and by N. by the compass, you will avoid the shoal, and fetch Dungeness Lighthouse.

Bring either of the three windmills on with the sea houses at East Bourne, and there is good anchorage in hard blue clay, and safer riding than at Dungeness.

Article VI.

On the upright Growth of Vegetables. By John Campbell, of Carbrook, F.R.S.E.

(Read to the Wernerian Society.)

To what physical cause are we to ascribe the upright growth of vegetables? It is owing, perhaps, to our familiarity with this appearance, and its obvious subserviency to the grand purposes effected by vegetable production, that so little attention has hitherto been directed to the solution of this interesting question. Ever present to our observation, invariable and noiseless in its progress, vegetation moves on unheeded. We admire the elegance of the slender flower, and almost reverence the grandeur of the lofty tree; but we think not of inquiring by what agency they are elevated above the mass of earth in which they germinate.

Some vague and gratuitous conjectures have indeed been long ago offered upon the subject. An affinity for air, for light, or some other favourite aliment, has been deemed sufficient explanation of the phenomenon; but such conjectures, directly contradicted as they are by facts, discoverable even on a slight examination, could never have maintained their ground, had the question ever been brought into general discussion. Even in the present day there are still some of our philosophers who are not weaned from their predilection for the agency of light; and on their account it may be proper to remark that, whatever may be the true cause of the upright growth of vegetables, it is demonstrable that affinity for light is not that cause; for, although light may be requisite for the vigorous growth of plants after a certain period, it has been found to be of no advantage, if not hurtful, to them in the earliest stage of vegetation; and, what is quite decisive on the point, is the fact that in all stages of vegetation plants will grow without light, though they do not grow vigorously; and as their growth, when

excluded from light, is still perpendicular, light cannot be the cause of their perpendicularity.

We do not, indeed, see every vegetable mathematically perpendicular; they are bent and twisted in every direction. But these deviations, it must be scarcely necessary to observe, are obviously the effects of adventitious circumstances; and it is presumed that it will be readily admitted that, by the general law of their nature, all vegetables grow upright.

It is evident that the agent which produces this universal feature cannot be an accidental principle, nor one dependant on any circumstance unconnected with the general structure of the plant; for, were this the case, the appearances would be discordant and contradictory. Neither can we resort for the cause to the simple fiat of the Almighty; for in that case the phenomena would be unvaried and unvariable, whereas we know that they may be varied at pleasure.* The cause, therefore, must be found in some agency connected with the process of vegetation; and the inquiry, to be successful, must be conducted by the principles of sound philosophy.

In looking for the cause of a universal effect, we must direct our attention to agents of universal operation; and when we perceive a universal coincidence between a cause and an effect, although we do not thereby obtain complete proof that the one is the cause of the other, we undoubtedly obtain that presumptive evidence, which requires only the addition of circumstances, by which we can understand how the one may be the cause of the other, to satisfy us that they do really stand to each other in that relation. It was under the influence of such views, and under the conviction that the general law which determines the perpendicular growth of vegetables could be connected only with a universal principle acting with a centrifugal force, that upwards of twelve† years ago it occurred to me that this coincidence was only to be found in gravitation.

The same conclusion was about two years afterwards deduced by Mr. Knight from a set of most ingenious and satisfactory experiments. One of these experiments, which of itself carries conviction, and to which I must afterwards refer, was the following:— Having constructed a variety of wheels, Mr. Knight fixed to their external circumferences a number of seeds of the garden bean, which had been soaked in water, and prepared for germination. These wheels, some in a vertical, and others in a horizontal, position, he connected with a water-wheel, by which the whole were

* Mr. Keith, in a paper lately read to the Linnæan Society, in despair of finding an intelligible cause, ascribes the ascent of the plumula, and descent of the radicle, to instinct. This solution is palpably inadmissible.

† Being then a pupil of Dr. Coventry, in his agricultural class, I communicated to him my opinion, illustrated by reference to the direction that would be given by gravitation to an icicle of hydrogen.

put in motion, with velocities varying from 80 to 250 revolutions in a minute. The result was exceedingly interesting. The seeds which germinated on the vertical wheel had the positions of their radicles and germens determined altogether by the motion of the wheel. The radicles protruded nearly straight outwards, whilst the germens in a few days encountered each at the centre. Extending beyond that point, the operating cause arrested their progress. They again returned, and met a second time at the centre. The germinations on the horizontal wheel, again, were differently affected. They were left exposed to the continued action of gravitation. The line of their progress, therefore, was the resolution of unequal forces. They were deflected ten degrees downwards from the plane of the wheel, the centrifugal force produced by 250 revolutions in a minute, in their circumstances, being to the force of gravitation as nine to one.

These results certainly establish the general principle; for they exhibit the uniform and exclusive effects of gravitation. Although, therefore, the conviction on my mind was previously complete, from the view of the coincidence between the cause and the effect, it is with much satisfaction I refer to Mr. Knight's experiment as affording the conclusive proof.

Did we know nothing more of the structure of plants, or the process of vegetation, we should still be bound to receive the paradox that vegetables grow upwards by virtue of a force drawing downwards; but we are enabled, though with much less certainty, to proceed somewhat further in this investigation.

Mr. Knight has offered a theory of the mode by which gravity operates in producing the upright growth of vegetables; and every thing which comes from such an acute mind deserves serious consideration. In the present instance, however, his theory, though, like all his suggestions, very ingenious, is evidently inconsistent with the phenomena of nature, and therefore must be erroneous.

But I do not consider myself entitled to throw aside the opinions of such an accurate observer without a detailed refutation. His hypothesis, compressed into a short proposition, seems to be this—that by the common principles of gravitation the sap accumulates in the under side of a deflected germen; that by this accumulation of the sap, and consequent increased growth of that part, the under side is elongated; that this elongation turns up the point, and that the perpendicular growth of plants is thus effected by a series of corrections.

Now it must be remarked, in the outset, that the leading fact on which Mr. Knight seems to found his opinion, viz. that the under side of a deflected germen does elongate, affords a very equivocal proof, indeed, of the inference drawn from it, that the perpendicular growth of that germen is the effect of this elongation; for, whatever be the cause of the *ascent* of the germen, it is demonstrable that, whenever it does operate on a plant which has been bent to one side, the under or shortest side must elongate. It could

not otherwise obtain the perpendicular direction. Even were external violent force applied, the effect would be the same. If we straighten an iron hoop, or uncoil a cable, the inner or shortest side must elongate more than the outer side, by the difference of the measure of their respective curves; and in like manner when a tree bends to the continued action of the wind, the upper, and not the under side, is elongated. The elongation, therefore, will result from any cause which, pushing upwards, shall most stretch the teguments of that side by which it is most retarded; and if so, it cannot be admitted as an evidence that it itself is the cause why the germen grows upwards.

Admitting, however, for the present, that an accumulation of sap takes place, and that there is a consequent elongation of the under side, still the principles of this hypothesis would by no means produce perpendicularity.

Mr. Knight seems to have assumed that as soon as the point is brought round to the perpendicular, the object being attained, the elongation will stop. But what ground is there for this supposition? Must not the elongation continue as long as the sap predominates in the under side? The turning up of the point does not affect this preponderance, which must still remain on the under side, for as yet nothing has been added to the opposite scale. The elongation of the fibres must still proceed, and the curvature produced by it continually increase; so that before the quantity of sap on both sides shall be equal, the point must have doubled back, and crossed the central line of the stem. But it would not stop even here—the equilibrium must be destroyed before the direction can be changed; it must, therefore, still continue to deflect, till it produce the same effects, and by reiterated and alternate elongations *every* plant would exhibit an appearance of contortions which, it is believed, no one plant has ever exhibited.

The small alternated deflection from the perpendicular, which, in many trees, is produced by the thickening of the wood at the alternate buds, cannot be mistaken for that waving line, which is here supposed to be a necessary consequence of the truth of Mr. Knight's hypothesis. An apt exhibition of this supposed appearance, and a strong evidence in favour of the accuracy of the inferences now drawn from these principles, is afforded by the beautiful experiment already quoted.

It will be recollected that the point to which the germens of the beans affected by the rotatory motion all tended, was the centre of the wheel; but they continued their growth in the same direction for some time after they had passed that point. Returning to the centre, they must, from the same influence, have again crossed it, and, after a short progress, again retraced their steps in the original direction. The stems of these beans, therefore, had they grown to any considerable length, must have been twisted backwards and forwards, the centre of the wheel forming the centre of the twists.

The tendency of a germen to point upwards in consequence of

the elongation of the lower side, cannot be supposed to have more influence in arresting its progress when arrived at perpendicularity, than the centripetal force to arrest the progress of the bean when its germen had reached the centre of the wheel; certainly its influence could not be so powerful; for by the hypothesis in question there is *no* principle which gives the germen a tendency to perpendicularity, that feature of its growth being stated to be the effect of the elongation. The point of the germen, therefore, would have no antagonist to contend with, in continuing its curvature past the centre of the stem. It would meet with no opposition till an accumulation of sap on the under side should operate so powerfully as to change its direction; and we must infer that the contortions produced by the alternate elongations would be much greater than those exhibited in the experiment, by the stems passing and repassing the centre of the wheel.

The hypothesis stands also contradicted by the facts we constantly observe in the progress of straightening young plants and shoots of trees. The elm, the beech, and some other trees, evolve at once almost the whole annual shoot. These shoots are then very tender, and of course pendulous. They straighten themselves, however, considerably during the first season, and generally, when well sheltered, approach near to the perpendicular in the course of the next. On the principles of Mr. Knight's hypothesis the process of straightening ought to commence at the top, where the point is turned up; yet every day's experience proves that they gain their perpendicularity by a general progressive change on the whole twig. By the elongation of the under side there is only provision made for tnrning up the point. No principle is furnished likely to operate in favour of the deflected stem. Indeed, the elongation of one side more than the other, instead of aiding a plant already curved to regain the perpendicular, seems better calculated to perpetuate the curvature.

But it may be questioned if there be in reality any such accumulation of sap, any such unvaried elongation of the under side of all deflected germens. It is obvious that, if it acts at all, it must act always, whatever twistings or contortions the continued elongation of one side only might produce. If the cause exists, it is situated beyond the reach of those sinister accidents which would affect or counteract a principle of external agency. Unless, therefore, we can recognise its influence in every case, we may conclude that we have still to discover the legitimate principle.

In young shoots the sap abounds in every part. We find, accordingly, that if a germen be prevented by any overpowering force from altering its position, it will grow, though confined to a horizontal direction, and both sides will elongate equally. It sometimes happens with the bean, and always with the onion, that the point of the germen remains long confined within the lobes of cotyledon after the germen itself has attained considerable length. From this circumstance it is bent into a semicircle, both ends being immersed

in the soil. The fern grows in this way also; and it is evident that, whilst growing in that position, it is the upper side of all these plants which elongate the most. A more common example is afforded by trees much exposed to the wind. They invariably elongate on the upper or windward side; and it may be thence laid down as a general rule that the trees of this country have their longest side to the south-west. I shall only adduce one fact more, somewhat similar in its nature, and equally decisive against the idea of an accumulation and luxuriance of growth being a consequence of deflection. The sap always flows most freely on the south side of trees exposed to the sun. It is where the sap flows most freely that the tree grows with the greatest luxuriance. Of course where the under side happens to be the north side, which in this country is most generally the case, the under side grows less luxuriantly than the upper side.

Thus all the phenomena exhibited in the growth of plants oppose themselves to Mr. Knight's hypothesis. The analogy of nature is equally arrayed against it. Unity and simplicity are the great characteristics of Nature's laws. The vast importance of the object attained by the perpendicular growth of vegetables, and the simple means by which that object is attained, form a beautiful illustration of this obvious remark. The provision that plants shall grow upwards secures for the animal creation the greatest possible quantity of vegetable food; and for effectuating this simple arrangement we expect to find an equal simplicity in the mechanism employed; to find some principle which directly communicates to plants an upright rectilineal direction. Under the influence of such a train of thought, we cannot for a moment believe that perpendicularity is the effect, not of any immediate agency, not of any directly buoyant or centrifugal force, but of a continued series of corrections.

In what manner, then, does the principle of gravitation act? To what part or circumstance is its influence directed? I am well aware that it is much easier to overturn an erroneous theory than to produce in its place one free from error; yet as the same reasoning which led me to the conclusion that gravitation was the operating principle, leads almost with equal force to a conclusion as to the particular mode by which that principle acts; and as the one has been already verified by decisive experiments, I am emboldened to hazard the other, which from these circumstances may lay claim to presumptive evidence in its favour.

In a former paper, which I had the honour to read to this Society, it was endeavoured to be explained *how* gravitation or attraction produced a perpendicular *ascent*; and it was stated as the universal law governing all perpendicular ascents and descents, "That as in the case of direct attraction (that is, where the bodies are at liberty to move in the line of attraction), the body most attracted leaves the body least attracted; so when attraction is resisted the reverse happens, the body least attracted leaves the body most attracted, but

the principles of gravity, which would find the greatest counter-action from the gravity of the plant, when the stem was in a horizontal position.

The effect here described may be produced by a buoyant principle bearing up the plant in which it is inclosed, as the hydrogen gas bears and presses upward the body of a balloon; or, by drawing it upwards, in consequence of its adhesion, as the balloon, rendered buoyant by the gas, carries up the car which is attached to it. In

On the Upright Growth of
VEGETABLES.

Fig. a.

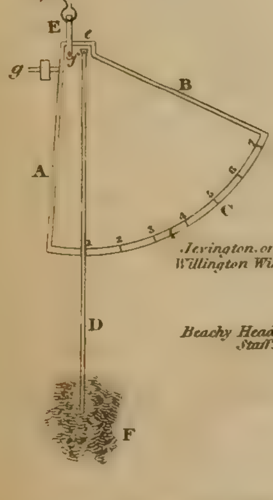


Fig. b.

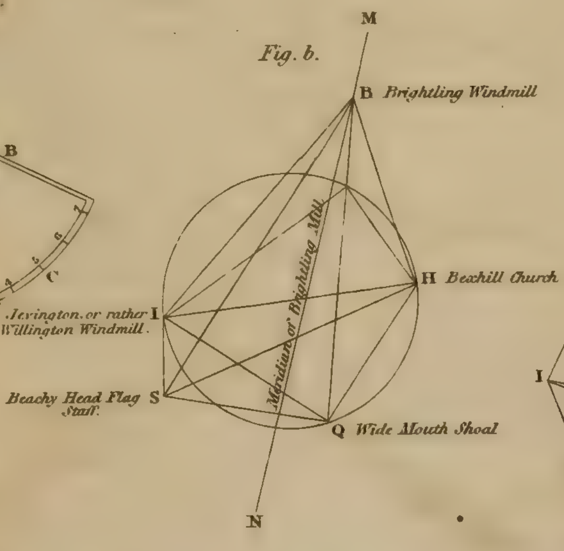
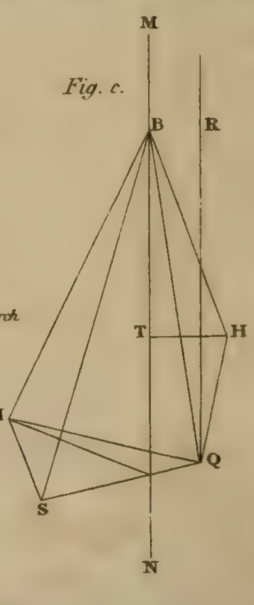


Fig. c.



Engraved for Dr. Thomson's Annals. — for Baldwin Cradock & Joy, Paternoster Row, June 1. 1816.

in an opposite direction." By this law the lunar and antilunar tides are raised; the lunar by direct, the antilunar tide by resisted, attraction; and when we recollect that no other cause is known whereby any particle of matter is made uniformly to ascend in a straight line from the centre, can we hesitate to adopt the opinion that this is the law to which we are to ascribe the upright growth of vegetables?

The most difficult part of the problem, however, remains behind—the investigation of those subtile influences by which this law operates on vegetables; and my confidence, though unshaken as to the general principle, diminishes as I proceed in offering an opinion as to the mode of its agency.

Let us premise some facts, the result of minute observation. Having in the month of June marked some young shoots of the spruce fir, the beech, and the thorn, which were all very much deflected, wires were fitted to their respective shapes, and by means of these wires I was enabled to delineate the different stages of straightening in all the twigs. (See Plate L.) After minutely examining them, I was satisfied that they had exhibited the same phenomena which would naturally have resulted from the mechanical operation of a great many threads attached to different points, and employed to elevate in parallel lines all the parts of the plants. In such a case the plant would gain the perpendicular position, not by any recurvature at the top, but by a general and progressive movement of the whole; the lower parts being affected, not only by the direct attraction of the threads attached to them, but also by their connexion with the upper parts, which would draw the lower ones along with them. The influence of gravitation, whether direct or resisted, may with much propriety be compared to the agency of a force operating by such parallel threads. They truly represent the lines of attraction; and as they represent also the agency by which these vegetables acquired perpendicularity, they exhibit to the mind a tangible idea of the principle which regulates the process.

Having watched the growth of a garden bean, and taken every morning an accurate sketch of its progress, the process of straightening from the semicircular position, in which the germen first appeared, till it attained perpendicularity, exactly corresponded with the observations made on the other plants, with this additional circumstance, that its progress seemed to be less rapid when its position was nearly horizontal, than when it was inclined either upwards or downwards; a circumstance accurately according with the principles of gravity, which would find the greatest counteraction from the gravity of the plant, when the stem was in a horizontal position.

The effect here described may be produced by a buoyant principle bearing up the plant in which it is inclosed, as the hydrogen gas bears and presses upward the body of a balloon; or, by drawing it upwards, in consequence of its adhesion, as the balloon, rendered buoyant by the gas, carries up the car which is attached to it. In

one or other, or both, of these modes, we may expect to find the buoyant principle operating in the upright growth of vegetables.

It is apparent that vegetables might be so constructed as, by an extrication of the lighter gases contained in the sap, to create a buoyancy, which, by pressing to the top of the plant, would tend to expand it in a perpendicular direction. Water, which serves so many purposes in vegetation, contains one of those ingredients which form carbonic acid; and the formation of carbonic acid, according to some of our best naturalists, is the great business of vegetable life. Such a decomposition of water must disengage the hydrogen, the greater part of which, however, seems to re-enter into immediate combination. Were that gas found in a free state, we could have no difficulty in ascribing to its agency the upright growth of plants. I have not learned that any experiments have been made to ascertain the existence of free hydrogen gas in plants, but I conclude that there cannot be any great quantity, otherwise it would have been incidentally noticed by those who were engaged in the decomposition of vegetable substances with other views. Having no proper apparatus, and being but an awkward experimenter, I cannot rely on the result of a slight attempt made by myself. Having raised a few garden beans when the stems reached the length of eight or ten inches, I cut them into three divisions; and squeezing the highest, middle, and lowest, parts of the germen under three glasses, placed in a chemical bath, I collected the gas from these parts. A small quantity was collected from the tops and middle parts; but as the only test I tried was to expose it to a flame, I could only conclude in general that it did not contain much hydrogen, because there appeared no inflammability. From these circumstances, however, and the important fact that there is another buoyant gas which certainly does exist in plants, we cannot with any probability ascribe the chief agency to the buoyancy of hydrogen.

It was for some time a favourite idea with me that the sap itself at different altitudes was of different densities; and thence, from the law of relative attraction, that it produced an upward pressure. The expression itself, too, (*ascent of the sap*) apparently carried in its own terms the solution of the question; for whatever might be the cause of the ascent of the sap, that ascent seemed to be a very good reason why the tubes in which it flowed did ascend. I am now satisfied that these notions were founded on erroneous views, they being inconsistent with the facts ascertained by experiment.

The luminous, yet condensed, view of the opinions relative to the ascent of the sap in vegetables, given by Dr. Thomson in his System of Chemistry, nearly demonstrates that the ascent of the sap is not affected by gravitation; and as experiment has proved that the sap itself becomes heavier as it ascends, it is evident that no buoyancy can be ascribed to it. The sap of vegetables seems to be analogous to the blood of animals, and the ascent of the sap, by which the nourishment is administered to the plant, may be paral-

leled to the circulation of the blood. The blood is impelled through every part of the body by no mechanical attraction or chemical affinity; but by a principle, *sui generis*, connected with animal life, and which, though its action may be increased by stimulants, and diminished by narcotics, cannot be communicated by any exertion of human power. The analogy between vegetable and animal life has certainly in some instances been carried beyond the bounds of sober reason, but no one can refuse assent to the existence of a connexion between these two orders of being, indicating a common principle in their organization. The circulation of the blood, therefore, produced by the irritability of the heart, leads us to ascribe the circulation of the sap to a similar organization in the vegetable; and as the examination of the sap vessels in trees does furnish evidence of a construction suited to such a purpose, we have good reason to conclude that this is indeed the true theory of that phenomenon.

The origin of that prejudice which would lead us to ascribe to the circulation of the sap the perpendicular growth of the vegetable will be found in the inaccuracy of the expression *ascent of the sap.* We now see that, instead of concluding that the germen is forced upwards by the ascending sap, we have better reasons to conclude that the sap ascends, because the plant stands perpendicular. Being absorbed by the roots, its course, if it flows at all, is necessarily upwards in an upright stem. But it flows in all directions, and with equal force in a trailing as in an upright plant. Like the circulation of the blood, which was destined to proceed with an equal course, unaffected by the position of the animal, the flow of the sap, intended to nourish all the parts of the plant, was made to depend on a principle altogether unconnected with those laws which, however powerful or extensive in their operations, are all essentially connected with the centre of gravity. Instead of *ascent,* therefore, we ought to adopt the expression *circulation* of the sap; and by the introduction of a term implying an indefinite direction, we shall extricate the question from that confusion which so invariably results from inaccurate descriptive expressions.

But if there be no principle as yet discovered to which, by an internal upward pressure, we can ascribe the full solution of the problem, the conviction that the cause is still to be found in the ascent, or descent of something connected with the plant, leads us to examine the external operations of vegetation, and in particular the constant and copious evaporation which seems essential to the existence of that process.

The quantity of water which is absorbed and evaporated by different plants has been the subject of many experiments. It has been gravely stated that the improvements in agriculture, by increasing the quantity of vegetables in this island, have so much increased the quantity of vapour as to have materially deteriorated the climate. Without adopting such an unpleasant opinion as this, we may safely credit the statements of well-informed naturalists when, from their

experiments, they inform us that, on an average, vegetables evaporate daily a quantity of water equal to half their weight. Spearmint was found to evaporate its own weight; and a cabbage plant weighing 1 lb. 9 oz. perspired 1 lb. 3 oz. It is scarcely to be doubted that water, which contains two of the elemental ingredients of vegetables, viz. oxygen and hydrogen, does by decomposition furnish nourishment to the plant; but its principal office in the progress of vegetation seems to be to serve as a menstruum for enabling the more fixed elements, carbon, lime, phosphorus, &c. to be imbibed by the roots, and circulated by the other organs. Having accomplished this important service, it unites with the excess of caloric, by which it is converted into vapour; and each lingering gaseous particle, separating with difficulty from its former companion, communicates to every part of the plant an upright direction, in which position it is permanently fixed by the formation and increasing inflexibility of the woody fibre.

It may appear surprising that such a quantity of water should be evaporated from vegetables, whilst their temperature remains not much higher than that of the surrounding atmosphere; but it is to this evaporation we must ascribe the low temperature which the plant preserves in the midst of circumstances calculated to raise it to a much higher degree. These circumstances have been developed by that ingenious naturalist Mr. Daniel Ellis, who has proved that during vegetation in all stages there is a combination of oxygen and carbon, "the formation of carbonic acid being the universal law of living or organized bodies, and seeming to be necessary to their life." When the oxygen thus combines, it parts with its caloric; and from experiment it has been found that the caloric evolved during the formation of 30 cubic inches of carbonic acid gas will melt 1 oz. of ice. That a large quantity of carbonic acid gas is daily formed from vegetables, will at once be apparent when it is recollected that, in consequence of the absorption of oxygen by this process, independent of what is formed from the decomposition of water, the presence of a few plants in a room, notwithstanding the supply from the ordinary circulating currents, soon renders the air unfit for respiration, and has not unfrequently been the cause of death. The heat thus produced would certainly be destructive of the plant, were it not carried off by the vapour; so that in the vegetable kingdom we find the same beautiful provision which, in the respiration of animals, protects the vital parts from the too powerful action of caloric.

We may hesitate, notwithstanding, to admit evaporation as a cause adequate to produce such an effect as the perpendicular growth of plants, though it is a step gained in our progress if it be admitted that in any degree it has that tendency. But we may feel re-assured, when we recollect that not only has evaporation the effect of producing buoyancy, in the particles adhering to the vapour; but that it is the only cause known connected with vegetation, which does certainly operate to produce such buoyancy. It

becomes us, therefore, to examine minutely all the circumstances connected with the action of this centrifugal force, as they regard the parts of the plant on which it acts, and the manner in which its force is exerted.

1. The process of evaporation goes on in every part of the plant, though chiefly in the tender parts, the young shoot, and the leaves. The bud, indeed, almost always protrudes the leaves first, and in many plants the leaves continue for a long time to envelope the stem. It must, therefore, be held that almost every particle of a plant, and particularly those particles at the top of the plumula, which are most soft and pliant, are exposed to the action of that centrifugal force, which is produced by the conversion of the particles of water attached to them, into gaseous vapour or steam; and therefore the effect does not depend altogether on the quantity evaporated in a given time, but on the quantity adhering to the plant, and the existing temperature by which the buoyancy of the gas is regulated.

2. We must contemplate each integrant particle of a plant to which a particle of gas adheres, as supported by its cohesion to the contiguous particles by which it is surrounded; so that, independently of any buoyancy derived from its connexion with the gas, it would hang, not downwards, but in a direction more or less inclined, according to the force with which it coheres to the contiguous particles. To the particles, in these circumstances, we have to apply the influence of the ascending vapour. Possessed of considerable buoyancy, or centrifugal force itself, the particle of vapour, whilst it continues to adhere to the particle of the plant, must communicate to it a portion of that buoyancy.

But, 3dly, The operation of this force in raising the particle of the plant to an upright position, must have the advantage of a lever power. The cohesion of the particles of the plant may be resolved into a succession of attractions from the centre to the surface of the stem. Each radius of this circle may be considered as a lever, having the central point as a fulcrum, or rather each particle in succession, may be considered as thus acted upon, the fulcrum of each being that point where the cohesive power is greatest, and which must always be the point nearest the centre. Even in the leaves something of this effect of cohesion will be found; for the footstalk of the leaf serves as its support, and as a medium by which any buoyancy produced in the leaf is immediately communicated to the twig or the stem.

Looking to each particle as thus acted upon, by a force which, though weak, is incessant in its operation; and recollecting what a small force is required to give perpendicularity to even a considerable weight, when supported at no great angle from the perpendicular, we have no reason to conclude that the cause now assigned must under any circumstances of the plant be inadequate to the effect. It is evident from Mr. Knight's experiment with the horizontal wheel that the circumstance of the centrifugal force

being directed only to particles maintained in certain positions by other causes, is very material to the present question. The force to and from the centre produced by the revolutions of the wheel would not have been sufficient to have sustained one twentieth part of the weight of the plants, either roots or germens; but these being supported otherwise, by their coherence to the parts resting on the wheel, even when the shoots at both ends far outstretched the limits of immediate support, the forces generated by the motion were sufficient to sustain the protrusions. It is not to be doubted that the controuling force in that experiment was that produced by the circular motion, which so distinctly arranged the parts of the plants in that position, which corresponded with their respective gravities. But it must be recollected that one effect of the revolutions of the wheel was to create a little atmosphere for itself, all the parts of which would be affected in their centripetal and centifugal forces. The direction of the buoyant gases, therefore, and the effects produced by them, would share the common lot, and have their tendency to ascend or descend in lines perpendicular to the centre of the wheel.

Another instance of the perpendicular elevation of heavy bodies by a force insufficient to sustain a single particle of these bodies, when applied to it in an individual isolated state, may be found in the tides. The moon's attraction is altogether inadequate to counterbalance the gravity of a particle of water, or to prevent its falling to the ground, yet, when it is so applied that one particle rests upon, and to a certain extent is thus supported by other particles, the attraction of the moon is found to be sufficient to sustain a column of water 10 feet high. Whilst we thus see the moon's attraction, which is not to be compared to the buoyancy of gaseous vapour, producing an effect which in the aggregate excites our wonder and admiration, is it too much to believe that, by communicating a part of its buoyancy to the individual particles to which it adheres, the constant evaporation which proceeds from a plant gives to it the tendency to perpendicularity.

Although I have thus ventured to suggest a principle for the solution of the question why vegetables grow upward, I am far from affirming that my opinion has been established by proof. This much, however, appears demonstrable, that evaporation goes on in all living plants with a never-ceasing activity, and that evaporation must, from its nature, communicate buoyancy, or a tendency to perpendicular growth. This buoyancy is the effect of that law of resisted attraction which produces the ascent of a balloon, the rise of water in a pump, and, as I have before stated, the antilunar tide. It is, indeed, the *only* cause known in nature which produces a direct centrifugal force, as the simple attraction on which it depends is the only known cause which produces a direct descent towards the centre. Whilst, therefore, I admit that much light must be thrown on this subject before we can have an accurate knowledge of the precise mode in which perpendicularity is pro-

duced; yet it appears to me that we have sufficient grounds for concluding that it is effected by the operation of buoyant gases.

Having arrived at this conclusion, it is almost impossible to withdraw the mind from contemplating, or to contemplate without admiring, the various combinations dependant on the law of gravity—so vast in their extent, yet so minute in their application. When the Author of the Sacred Volume wishes to impart a sublime idea of infinite power, he tells us that God said, "Let there be light, and there was light." To the philosophic mind, which contemplates that mighty influence which binds the spheres, that universality which pervades all nature, and that wonderful adaptation to almost every varied purpose of utility, it must appear that there was a display of infinite wisdom equally sublime when God said, "Let matter gravitate."

Article VII.

A Comparison of Albumen with Gluten. By H. F. Link.*

Fourcroy first remarked the presence of albumen in vegetables. The expressed juice of cresses, cabbage, scurvy grass, deposited, on standing, a greenish substance, which, when boiled, precipitated a yellowish-grey flocky matter very similar to animal albumen. Afterwards albumen was detected in a variety of vegetable bodies. Vauquelin found it in the sap of the carica papaya, Einhof in potatoes, Schrader in cabbage, Grotthus in the pollen of tulips, &c.; not to mention the observations made during the preparation of sugar from beet. From all this it appears indisputable that a substance is found in plants similar to animal albumen. Proust alone has hesitated about applying this name to vegetable albumen, and considered the matter as at least very much mixed with gluten.

All that is precipitated from the sap of plants by boiling in flocks always appeared to me so similar to gluten, that I have not been able to prevail on myself to consider it as a peculiar substance. There is, indeed, this difference, that the former substance is found dissolved in the sap of plants, and is only brought into a solid state by heat, while the gluten exists already in a solid state in wheat flour. But this is not sufficient to constitute distinct species. We can only say that in wheat flour the same substance occurs in a solid state which is found liquid and in solution in the sap of cabbage. I consider it, therefore, as necessary to compare coagulated albumen with the gluten of plants, in order to determine whether the albumen of plants bears the greater resemblance to animal albumen

* Translated from Schweigger's Journal, vol. xiv. p. 294, October, 1815.

or to gluten. I employed, therefore, common albumen from eggs hard boiled, and gluten washed clean from wheat flour, and made with them the following comparative experiments:—

1. Albumen is insoluble in water. The water becomes muddy by continued boiling. Albumen dried, especially by artificial heat, is much more soluble in water.

Gluten is likewise insoluble in water. The liquid becomes muddy by continued boiling. Dried gluten is much more soluble.

2. Absolute alcohol does not dissolve albumen. When allowed to stand upon it for some time, the liquid becomes muddy. Alcohol likewise becomes muddy when boiled with albumen.

Absolute alcohol does not dissolve gluten. When allowed to stand upon it, it becomes muddy. It is rendered muddy, likewise, by boiling along with it.

3. Very dilute sulphuric acid does not act upon albumen. Boiling merely separates it into smaller parts. Strong smoking sulphuric acid gradually dissolves the greatest part of the albumen without the assistance of heat. The solution is brownish-yellow, and muddy. When dropped into water, a white precipitate falls similar to fresh albumen. The residual albumen is blackish, but transparent. Strong sulphuric acid, when assisted by heat, chars albumen.

The solution of albumen in sulphuric acid precipitates muriate of tin, sulphate of copper, and proto-sulphate of iron, of a brownish-white colour.

Dilute sulphuric acid does not act upon gluten. By boiling, it is only more minutely divided. Strong smoking sulphuric acid partly dissolves gluten without the assistance of heat. The solution has a dark, almost black, brown colour, with a slight tint of blood-red. When dropped into water, a yellowish-grey precipitate falls in flocks similar to gluten.

The solution of gluten in sulphuric acid precipitates muriate of tin, sulphate of copper, and proto-sulphate of iron, of a dark brown colour.

4. Cold nitric acid gives albumen a yellow colour. By boiling, it dissolves it, and the solution is yellow, and quite transparent. When a solution of albumen in six times its weight of nitric acid is evaporated, oxalic acid is obtained, but in small quantity.

Cold nitric acid gives gluten a yellow colour. By boiling, a yellow-coloured solution is obtained, which remains somewhat muddy. The undissolved matter, being separated by the filter, and dried, was in flocks, which burnt very easily. By evaporation with six times its weight of nitric acid, oxalic acid was obtained in considerable quantity.

5. Cold muriatic acid does not act upon albumen. When assisted by heat, a solution takes place, which has a blackish brown colour. Some flocks remain behind undissolved.

Gluten is only dissolved by hot muriatic acid. The solution has a brown colour, and some flocks remain undissolved.

6. Acetic acid (*acetum concentratum* of the Prussian Pharmacopœia) does not dissolve albumen, even when boiled upon it.

Nor is gluten dissolved by the same acid in similar circumstances.

7. Caustic soda acts upon albumen, and forms a yellowish solution. But the application of heat is requisite in order to obtain a complete solution. It is brownish-yellow, and transparent; and when saturated with an acid, it lets fall a white precipitate similar to albumen. When poured into alcohol, a white matter falls, mixed with carbonate of soda. On cooling, the solution lets fall a dark brown matter.

Caustic soda likewise dissolves gluten, and the solution is yellow. But heat is requisite in order to obtain a complete solution. It is brownish-yellow, and somewhat muddy. When dropped in alcohol, a yellowish-grey matter precipitates, containing carbonate of soda. Acids, likewise, throw down a yellowish-grey matter. When the solution cools, a yellowish matter subsides.

8. Ammonia does not dissolve albumen, even when boiled on it. But it became whiter, more transparent, and bulky.

Gluten was not dissolved, though boiled in ammonia; but it became whiter, and fell to pieces.

9. Albumen, when distilled, gave out ammonia and an empyreumatic oil. When heated with potash, the smell of ammonia was not perceptible.

Gluten likewise furnished ammonia and an empyreumatic oil, when distilled; nor did it give out the smell of ammonia when heated with potash.

From these experiments it is obvious that gluten and coagulated albumen do not differ much from each other. Gluten, with nitric acid and soda, forms muddy solutions; albumen, transparent ones. Gluten forms with strong sulphuric acid a dark brown solution, with a shade of blood-red. Albumen forms a solution which has a yellowish-brown colour. Sulphate of albumen precipitates some metals white. Sulphate of gluten throws them down brown. The former is precipitated by water, white; the latter, grey. Gluten gives more oxalic acid than albumen, when treated with nitric acid. The two substances do not differ so much from each other as different varieties of resin or gum. I think, therefore, that both should be placed under the same genus. Albumen occurs in many animal bodies in a coagulated state, while in others it is liquid. The same remarks apply to the state in which gluten occurs in vegetables.

I now separated a quantity of vegetable albumen from the sap of white cabbage (*weisskohls*). This sap let fall a green precipitate, which possessed the properties of the green matter of vegetables. When treated with alcohol, a grey flocky substance remained undissolved, very similar to albumen prepared by boiling. The filtered sap became muddy in the air; and, when boiled, deposited a grey matter in flocks. Water did not dissolve this matter, but became

muddy when boiled over it. Alcohol treated in the same manner became likewise muddy. Strong smoking sulphuric acid dissolved it without the assistance of heat, and formed a dark brown solution, darker, and with a stronger tint of red, than the solution of animal albumen, but not so blood-red as that of gluten. The solution in nitric acid was likewise somewhat muddy, and furnished more oxalic acid than animal albumen, but less than gluten. Pure soda became yellow when poured over it, but did not dissolve it till assisted by heat. On cooling, it let fall a yellowish deposite. The solution in sulphuric acid precipitated muriate of tin and sulphate of copper of a brownish-yellow colour.

The result of these experiments is, that coagulated albumen and gluten differ very little from each other, and belong to the same genus of vegetable substances. Vegetable albumen from the sap of the white cabbage is a substance intermediate between the two in its properties, but approaches rather nearer to gluten than to animal albumen.

Grotthus has truly remarked that there is a species of starch which approaches very nearly to albumen. He found it in the pollen of tulips; and it is a pity that he made no microscopical observations on it. From the expressed juice of unripe gooseberries I obtained a sediment of a green colour. By means of boiling alcohol I separated the green substance from this. It exhibited the properties of the common colouring matter of plants. The undissolved residuum had a bluish-grey colour. When viewed through the microscope, it appeared to consist of transparent grains, mixed here and there with soft flocks. Water and alcohol did not dissolve this substance by boiling, but they became muddy. Soda formed a dark brown solution when assisted by heat. On cooling, flocks precipitated. The solution in sulphuric acid was reddish-brown, and precipitated muriate of tin, sulphate of copper, and proto-sulphate of iron, brown. The nitric acid solution remained muddy, even when strongly heated. Flocks were deposited from it, and much oxalic acid formed. Acetic acid formed no solution. From these experiments we see that this species of starch approaches very nearly to gluten. The grains in plants usually called starch are of various kinds. We find them, 1. Gummy, as what I have described in althæa mucilage. 2. Starchy, as the common starch of wheat, potatoes, &c. 3. Gluteny, as the kind which I have just described.

Article VIII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday, April 25, there was read an Appendix to a Paper on the Effects of Colchicum Autumnale on Gout, by Sir E. Home.

In a former paper Sir Everard endeavoured to prove, first, that this medicine can be received into the circulation without permanent injury; and, secondly, that its beneficial effects on gout are produced through that medium. He judges of the influence of the medicine from its effect on the pulse, which is diminished 10 or 20 beats per minute in about 12 hours after it is taken. As this effect is produced when the same medicine is injected into a vein, he draws as a consequence that in the former case the diminution of celerity is owing to its arrival into the circulation, and not upon the state of the stomach. He was afterwards induced to try whether the effects of a larger quantity injected into the veins would also correspond with those of an over dose taken into the stomach. 160 drops of the infusion of colchicum were injected into the vein of a dog. He lost all power of motion, the breathing became slow, and the pulse hardly felt. At first it became slower, afterwards was accelerated. The animal was purged and vomited, and in five hours died. The stomach and intestines were found inflamed.

On Thursday, May 2, there was read a case of complete aphonia, cured by the medical application of the electric fluid by Augustus Sayer, M. D. The subject of this case was a young man who from his infancy had some difficulty of articulation, arising apparently from the size of his tongue. Being an officer in the French service, and exposed to the fire from a Prussian battery, a cannot shot, which killed two men near him, stunned him also, so that he fell at the same time with them. He remained for some time in convulsions, with a very laborious respiration; but, when examined, had no wound. Next morning he began to be sensible, but had no recollection of what had passed. Vision remained unimpaired. The sense of smell was preternaturally acute; but his hearing was nearly, and his taste entirely, destroyed. The motion of the tongue was so very imperfect that he had lost all power of articulation, and was scarcely able even to swallow. The sense of feeling was also affected throughout the whole of the left side. Spirituous liquors and coffee (especially) put his body in the most violent agitation, and even the smell of coffee had a very great effect on him.

Various nervous medicines and the warm bath were tried to no purpose; so that, after being carried from town to town, during the five following months of the campaign, he was invalided, and returned to his friends at Brussels. Here electricity was recommended, and tried first as an aura, then in sparks, and at last in shocks. The effect of the third shock was so severe that he was with difficulty prevailed upon to endure any more. On the seventh, his tongue was suddenly set free, as if a cord had snapped asunder, and he recovered his speech, and was as well as before the accident.

At the same meeting a paper by Dr. Wollaston on the cutting diamond was read. Having never met with any satisfactory account of this property of the diamond, Dr. Wollaston endeavoured to determine how it acts by experiment. The diamonds chosen for this purpose are naturally crystallized with curved sur-

faces, so that the edges are also curvilinear. In order to cut glass, a diamond of this form requires to be so placed that the surface of the glass is a tangent to the curvilinear edge, and equally inclined laterally to the two adjacent surfaces of the diamond. Under these circumstances the parts of the glass to which the diamond is applied are forced asunder, as by an obtuse wedge, to a most minute distance, without being removed; so that a superficial and continuous crack is made from one end of the intended cut to the other. After this any small force applied to one extremity is sufficient to extend this crack through the whole substance, and successively along the whole breadth of the glass. For since the strain at each instant of the progress of the crack is confined nearly to a mathematical line at the bottom of the fissure, the effort necessary for carrying it through is proportionally small. He found by trial that the cut caused by the mere passage of the diamond need not penetrate so much as $\frac{1}{200}$ of an inch. Other mineral bodies recently ground into the same form possess the same property, but soon lose it.

On Thursday, May 9, a paper by William Chapman, Esq. M.R.I.A. on the probable formation of mineral coal, and on the position and accompanying circumstances of fossil trees, was read. He was led into the train of reasoning exhibited in the paper by viewing several of the casts of trees so common in the sandstone which accompanies the coal in Northumberland. These casts are composed of sand-stone, and are surrounded with a layer of charry matter, which is conceived to have been the bark of the tree. Similar trees in the same position occur in peat bogs. From this, and from the remains of reeds and other similar vegetables common in the coal beds, Mr. Chapman conceives that the coal originated from peat beds, and that it acquired its present consistency and compactness from compression. He enters into several calculations to show the degree of compression that would be necessary to produce the effect, and conceives that by the action of matter in fusion on the top of the beds, the charring may have been produced requisite to convert the coal into Kilkenny coal and Welch culm.

On Thursday, May 16, a paper by Mr. Morney was read, describing a mass of meteoric iron found by him in Brazil in the year 1810. At that time a pretty strong chalybeate was discovered not far from the seat of Government. This discovery was considered as important, and induced the Prince Royal of Portugal to request Mr. Morney to take a journey to examine some thermal springs which had been announced as existing several years before at the distance of about 50 leagues from Bahiar. As a further inducement to undertake this journey, he was told of a remarkable stony mass which had been observed at no great distance from the thermal springs. This body had been first remarked by a shepherd. The superintendent of the neighbouring district was sent to examine it. His report was so wonderful that orders were given to remove the stone, and bring it to the seat of the Court. The attempt was made. It was raised upon an ill-constructed carriage, and 40 pair

of oxen were yoked to it; but they were not able to drag it along. It was, therefore, abandoned; and Mr. Morney still found it placed on the carriage. It was magnetic; had distinct north and south poles; and was obviously crystallized in its texture. Its weight was estimated at about 14,000 lbs. It was seven feet long, and four feet broad; but its thickness was irregular. Mr. Morney estimated its solid contents at 28 cubic feet. From his experiments on the spot he considered it as iron, but thought that it gave indications of containing nickel. It rusted rather faster than common iron.

At the same meeting a chemical examination of a specimen of this mass of iron by Dr. Wollaston, was read. His method of detecting the presence of nickel was this. He dissolved about $\frac{1}{100}$ of a grain in nitric acid upon a watch-glass, evaporated to dryness, and added a drop of ammonia to dissolve the oxide of nickel. This solution was drawn by means of a glass rod to a little distance from the oxide of iron, and a drop of prussiate of potash added. The presence of nickel is detected by the milk-white colour that ensues. To determine the quantity of nickel he dissolved 50 gr. of the iron in nitro-muriatic acid, added an excess of ammonia, and then evaporated the solution nearly to dryness. Ammonia was digested on the residue. A blue solution was obtained. This was saturated with sulphuric acid, evaporated to dryness, and the dry mass exposed to a heat sufficient to drive off the sulphate and muriate of ammonia. The residue was re-dissolved in water, and crystallized. The crystals weighed 8 gr., indicating 2·93 gr. of nickel, or nearly four per cent. of that metal in the iron mass.

GEOLOGICAL SOCIETY.

March 15.—A notice on the beds of gravel in the neighbourhood of Lichfield, by Mr. Arthur Aikin, was read.

The principal object of this paper is to describe the state of decomposition exhibited by the rolled pebbles which occur in this gravel. They are all, except the quartz pebbles, more or less altered; but the chalcedonies in the form of concentric agates and of coralloidal agates, exhibit the most remarkable changes. In the former the chalcedonic zones are in many cases entirely converted into a white earth, while the nucleus of quartz remains untouched: and in the latter the tubipores, being of quartz, remain, while the interstitial chalcedony has been in some cases totally, and others partially, removed.

A paper on the paramoudra, a singular fossil body, found in the chalk of the north of Ireland, by the Rev. W. Buckland, Prof. Min. Ox. M. G. S. was next read. The name paramoudra is applied by the inhabitants of Belfast to a fossil, which occurs in all the chalk pits between that place and Moira, and which has also been found at Whitlingham near Norwich; specimens from thence having been sent to the Society four years ago by the late Dr. Reeve of Norwich. Its form is more or less that of an oblong gourd: it is composed of flint, and is the longest fossil that occurs

in the chalk, the length varying from one to two feet, and the breadth from six to 12 inches. A tubular cavity, open always at one end, and often at both ends, passes in the direction of its long diameter. No appearances of organic structure have been detected, but it appears from its external figure to have been one of those supposed spongy bodies, which are of such frequent occurrence in chalk flints.

April 5.—A communication from the Foreign Secretary was read, giving an account of certain aerolites that have recently been examined by M. M. Gillet de Laumont and Schreiber. From this examination it appears, that the well known areolite which fell at Stannern in Moravia, contains no metallic iron whatever; but a larger proportion of alumine and of lime, than has hitherto been noticed in substances of this kind. On the other hand, the mass weighing 190lbs., which fell near Egra in Bohemia, appears to be wholly composed of metallic iron.

A paper by Robert Stevenson, Civil Engineer, entitled "Observations on the general bed of the German Ocean and British Channel," was then read. The object is to show that the bed of the German Ocean and of the British Channel is gradually, but very perceptibly, filling up, in consequence of which, the level of its water is continually rising. This is proved, in the opinion of Mr. Stevenson, from the circumstance that not only the headlands and exposed parts of the coast are wearing away, but yearly and important encroachments are making on the shores of sheltered bays, far within the Frith of Forth, and in other analogous situations, which cannot be attributed to any other cause than the actual overflowing of the water of the Ocean.

ROYAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Royal Institute of France during the Year 1815.

Physical Department.—*By M. le Chevalier Cuvier, Perpetual Secretary.*

(*Continued from* p. 396.)

ZOOLOGY, ANATOMY, AND PHYSIOLOGY.

The sciences are not strangers to true erudition. If it has sometimes happened that an attentive perusal of the ancients has excited philosophers to observations which have led to important truths, it is not uncommon, likewise, for fortunate observations of philosophers to throw an unexpected light on obscure passages in the ancients. Some notes of M. Cuvier on the books of Pliny relative to animals furnish a proof of this. Thus M. Cuvier supposes that the lynx of the ancients mentioned as coming from warm climates was not our *lynx*, but the *caracal*; and he shows that the caracal

possesses all the properties ascribed by the ancients to their lynx. The *leoncornutte* and the *catoblepas*, two animals to which the ancients ascribed a monstrous conformation and noxious qualities, appear to him to be only the result of bad descriptions made by ignorant travellers of that animal in the interior of Africa, to which travellers have given the name of *gnu* (*antelope gnu*, Linn.), whose singular shape, fierce look, and the pointed hair on its snout and mane, must have often rendered it an object of terror.

Of the five unicorns spoken of by the ancients, M. Cuvier supposes that the first four, the ass of the Indies, the unicorn horse, the unicorn ox, and the monoceros, properly so called, are merely the rhinoceros differently disfigured by the relations of travellers and merchants.

He shows that all which the ancients have said of the *asp* of Egypt, the *asp* by excellence, belongs completely to that species of viper with a large neck called *coluber haje*, the history of which is so well related by Geoffroy in his great work on Egypt.

He reconciles the contradictions of the ancients respecting the dolphin by showing that they gave that name to two very different animals; one, which is our present dolphin (*delphinis delphis*, Lin.); another, which belongs to the genus of sharks or sea dogs.

The greater number of the fables respecting the hyena and ichneumon are explained by the singularity of their conformation; even the pretended continuity of the vertebræ of the neck in the hyena is in some measure true. The extreme rigidity of the muscles of the neck frequently occasion ankyloses between the cervical vertebræ, and M. Cuvier has observed examples of it.

All the world knows the little animal called *musaraigne* or *musette*, which has a considerable external resemblance to a small mouse, only that its snout is more pointed, and its ears much smaller. But though it has been examined and dissected by several naturalists, all the peculiarities of its organization had not been observed. M. Geoffroy St. Hilaire has just discovered that on each side under the skin there is a peculiar gland, which exudes a glutinous matter by a series of pores surrounded with hairs, which are larger and stiffer than on the rest of the body, and which may easily be perceived by touching the body.

M. Cuvier, who has resumed his researches into the anatomy of the molusca, has this year read to the Class a memoir on the anatifes and balanes; and another on several genera of shells approaching the patellæ, the oscabrions, and the haliotides.

The anatifes and the balanes presented organs of generation and a nervous system very different from what is observed in the molusca in general. The nervous system, as well as the jaws, would bring these animals near to the insects.

The haliotides, the patellæ, and the oscabrions, have other singularities. Their sexes are not separated, as it is in the buccinæ and other aquatic turbinated animals. Nor are they united so as to require a reciprocal fecundation, as the snails and the aplysia, but

their hermaphroditism is complete, so that they suffice themselves like the oysters, and many other bivalves.

M. Cuvier has likewise given a memoir on the ascidiæ, a kind of molusca enveloped, not in a shell, but in a cartilaginous crust, fixed to the rocks, and provided with two openings, one of which receives and rejects the water necessary for respiration, and the other affords an outlet to the ova and excrements. A great cavity covered with a fine vascular net, which comes in place of the branchiæ, receives this water, and with it the corpuscula with which the animal is nourished. At its bottom is the mouth, which leads to a sort of gizzard. These animals have a heart, liver, and nervous system, pretty similar to that of other molusca. But the relative disposition of these parts, as well as the form and surface of the external envelope, varies much, according to the species.

This anatomy of the ascidia was so much more to the purpose, because it served to elucidate observations of a much more novel and important nature, which were made almost at the same time on animals of a similar nature, by M. Savigny, Member of the Institute of Egypt.

No compound animals are yet known except those in the order of polypi. All the corals, madrepores, sea pens, and a great many alcyonia, appear merely aggregations of different polypi united in an intimate manner, and whose nourishment takes place in common, so that what one eats is of use to all, and they all seem animated by a common will. This last circumstance is certain at least with sea pens, which transport themselves from one place to another by the combined and regular rowing of thousands of little polypi which proceed from all the feathers. The structure of these polypi is sufficiently simple for the imagination to conceive this kind of association, which we may in some measure compare to the different branches of the same tree.

But M. Savigny has discovered compound animals of another kind, and whose individual organization is much more complicated. They bear a singular resemblance to those molusca called ascidiæ, which themselves have some analogy with the animals of bivalve shells. We find in them equally a bronchial sack, which the aliments are obliged to traverse to arrive at the mouth; a muscular stomach; an intestine, of which the rectum mounts towards the side of the mouth, and forms there a second orifice; a nervous ganglion placed between the bronchial orifice and that of the anus; an ovarium, and an oviduct. In fact they are real ascidiæ united in masses by a common flesh, and partaking, in consequence, of a common life. This sort of animal aggregation had been hitherto confounded with the alcyoniæ. They are numerous; and M. Savigny, who has described and represented them with details to which their singularity entitled them, has observed a sufficient number of forms in them to constitute eight genera.

Among these compound animals some form fixed masses more or less irregular, as is the case with many alcyoniæ; others are ranged

in stars round a common centre; and these naturalists, considering each star as a simple animal, have called botryllæ; others in fine are combined in innumerable quantities, to form by their assemblage a long hollow cylinder, open at one end, which moves like the sea pens, and which Peron, the first person who perceived it, considering as a simple being, called *pyrosoma.*

MM. Desmarets and Lesueur had made on these last two genera observations quite analogous to those of M. Savigny, and which have fully confirmed them.

There exists among those great zoophites to which the ancients gave the name of *free nettles of the sea,* a genus which the Danish naturalist, Otho Frederick Müller, has made known, and called lucernaria, because he found in it I know not what resemblance to a lantern. Its general form is a hollow cone; at the centre of the base is the mouth; and from the edges of that base proceed arms usually eight in number, charged with small tentaculæ, sometimes at equal distances, sometimes connected in pairs.

M. Lamouroux, Professor of Natural History at Caen, has carefully watched an animal of this nature with eight equally distant arms, of a pale rose colour, dotted with red, and having eight red bands penetrating into the base of the arms, and which are the cœcums or intestines. These eight organs communicate with a central stomach. Each of them is lodged in a particular cavity, in which a kind of mesentery retains it. The kind of life of the lucernariæ seems to resemble that of the actiniæ, or sea anemonies.

The same naturalist has presented to the Class a new compendium of his general work (of which we have before spoken) of these kinds of compound zoophites, whose trunks are not stony, or, as he calls them, *flexible coralligenous polypi,* such as the sertulariæ and the flustræ. The profound attention which he has paid to these animals in general has enabled him to remark distinct characters sufficiently remarkable to form nearly 50 genera, which he has divided into 10 families, and under which he has arranged 560 species, almost the half of which are new.

We can only repeat the wish that this great work may be speedily published.

M. Leclerc de Laval, the same person who examined the confervæ, has presented to the Class some interesting observations on some microscopic animals. One of them, which M. Leclerc has discovered, and called *diflugia,* is scarcely the tenth of a line in diameter, is enveloped in a membranous case formed of very fine sand, and from which a kind of arms issue, which are merely an extension of its substance, and of which the number, the form, and the proportions, vary almost at pleasure. This animal ought to have an analogy with that which Ræsel called *proteus,* and which likewise assumes in a few minutes a thousand different forms.

The other animal observed by M. Leclerc is a hymenopterous insect, discovered by M. Jurine, Correspondent of the Class, and called by him *psile debosc,* but which belongs to the genus *diapria*

of Latreille. It has at the base of the abdomen an elevated horn, going forwards as far as the head, where it terminates by a bend back. M. Latreille has ascertained that this horn is the sheath of the wimble, an instrument with which many other hymenoptera are furnished, but which is usually differently placed. The base only of the wimble of the diapria is contained in its horn; but the point issues, as usual, from the anus.

M. Latreille has given us a very detailed description of certain crabs of the Mediterranean, very remarkable from having their eyes placed, not, as in the ordinary crabs, upon a single moveable articulation, but upon a long tube with two articulations, so that the animal moves them like the branches of a telegraph. Their hind feet are placed upon the back, like those of the *dorippes*. Some of these crabs had been already observed by Rondeletius and Aldrovandus, but these ancient naturalists did not mention the singular structure of their eyes. M. Latreille forms them into a genus, under the name of *hippo-carcinus*. Almost at the same time Dr. Leach, a skilful English naturalist, who is occupied with a great work on crustaceous animals, described these species under the generic name *homolus*.

M. Savigny has established last year, by many observations, an analogy of structure much greater than was supposed between the mouths of winged instruments, whether suckers or masticators; and he has shown that the sheaths of the suckers, trumps, and other instruments of deglutition of the first, and sometimes these instruments themselves, may be considered as prolongations of some of the palpæ and jaws of the others. He has presented this year a great work, from which result analogies of another order, between the mouths of the ordinary masticators and those of certain genera which appeared anomalous, some of which have been arranged among crustaceous animals, and others among insects without wings.

Naturalists had remarked for a long time that a part of the jaws of these genera with extraordinary mouths resembled feet, and M. Savigny endeavours to prove that they are really feet, which, assuming more or less the form and the functions of jaws, join themselves to the jaws properly so called, or even take their places altogether.

Thus in the scolopendræ there exist two sorts of supernumerary lips, of which the outermost have strong, hooked palpæ, which serve the animal for seizing his food. M. Savigny observing that they are not connected with the head, but with the first ring of the body, considers them as the first two pair of feet metamorphosed.

In the crabs, in which the head and corselet are confounded together, the supernumerary jaws are evidently the first feet; frequently their form even, as in the squillæ, is pretty evident. But in these animals, and in several others, whose mouth the author has described with infinite attention, there always exist ordinary jaws. On the other hand, in spiders, scorpions, and other genera without

antennæ, there remains scarcely any trace of head, and the true jaws have disappeared. There exist only supernumerary jaws, that is, feet converted into jaws.

Such is a short sketch of a very original work, the proofs of which have for their basis observations so long and so numerous, that we cannot find room for them in our analysis.

M. Delabillardiere, who continues to observe his bee-hives, has made some new observations on that subject, so admirable and so inexhaustible.

It is known that, after the last swarm has left the hive, the working bees, similar in point of ingratitude to many other more elevated beings, make haste to rid themselves of the males, which are no longer necessary for propagation, and the support of which would consume a great deal of provisions. They make a dreadful carnage of them; but if we are to judge by the expressions of some authors, this business is always terminated in a few days. In fact, however, it lasts several weeks. When the hives are weak, or contain but few working bees, the operation lasts a much longer time. The males are even altogether spared in those hives that contain no queen, or when the queen, as sometimes happens, produces only males. M. Delabillardiere gives a detailed example of this rule, already recognized by Huber. Those who keep bees, therefore, may discover by the great number of these drones remaining in the hive after the usual time, that no more new swarms are to be expected from it, and that the hive may be rifled without inconvenience.

Every body knows the noise similar to that of the balance of a pendulum, which has long inspired the superstitious with terror, and which has received the name of death-watch. Naturalists soon supposed that it must proceed from an insect. Some have ascribed it to a spider; others to the little animal wood-louse; and others to the little coleopterous animal called *vrillette*, because it pierces wood with an instrument like a drill (*vrille*). Among those who have adopted the last opinion, some ascribe the noise to the perfect animal, others to its larva; and all have supposed that it produced the noise while boring the wood, either as food, or to make its way through it. M. de Latreille had observed that the vrillette makes the noise, not while boring wood, but by striking. M. Delabillardiere has established the same fact by a series of observations; and as he made them on a female, he supposes that the object of the noise is to call the male, as is the case with many other female insects at the time of propagation.*

* All this, and a great deal more, was long ago described by Allen and Derham in the Phil. Trans. vol. xx. p. 376; vol. xxii. p. 832. This animal is the ptinus pulsator of Linn. Derham describes and figures another animal that also beats. Phil. Trans. vol. xxiv. p. 1586.—T.

(*To be continued.*)

Article IX.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

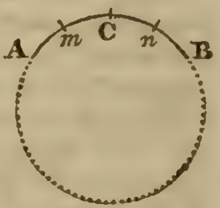
I. *Fulminating Platinum.*

Mr. Edmond Davy, Professor of Chemistry to the Cork Institution, has recently discovered a fulminating compound of platinum, which has some curious properties. He is at present engaged in examining this substance, and will shortly make known the results of his investigation. This peculiar compound explodes at a moderate heat. This effect is accompanied by a flash of light. The substance is completely decomposed, and resolved into metallic platinum and gaseous products. When the fulminating platinum is put into liquid ammonia it is partially decomposed, and a quantity of gas is evolved. In ammoniacal gas, this substance becomes ignited. When it is moistened with alcohol a slight crackling noise is produced. The substance scintillates, and burns with a red flame.

II. *Demonstration that no Part of a Circle is a straight Line.*

A Correspondent, who subscribes himself A Constant Reader, has sent me the following demonstration that no part of a circle is a straight line:—

In the arc A B of a circle take a small part, A C, which suppose to be a straight line. But if A C is a straight line, C B, which is equal to it, must be a straight line. For the same reason *n m*, which includes the point in which A C and C B are joined, is also a straight line; and therefore the whole *arc* A B is a straight line, which is evidently absurd. Therefore no part of a circle is a straight line.—Q. E. D.



Whether any part of a circle is, or is not, a straight line, is a point of difference between some of our most celebrated mathematicians. On the assumption that a circle is composed of a number of small straight lines, Hutton, Leslie, and several other great geometers, have built some very important theorems. On the contrary, an ingenious writer in the Quarterly Review for 1810, with several others, declaim strongly against the principle. Simpson and Legendre have likewise discountenanced it. The subject is, therefore, of considerable importance; and the above demonstration appears to me satisfactory. U.

III. *Iron Tube Barometers.*

(To Dr. Thomson.)

SIR,

Seing in your *Annals of Philosophy* that Professor Playfair claims the invention of making barometers with an iron tube and a float,

I beg leave to inform you that I have one in my possession that has been made near 20 years since, which you may inspect at any time.

I am, Sir, your most obedient humble servant,

H. Cary.

182, *Strand, April* 13, 1816.

IV. *Fermenting Heat.—Steam.*

(To Dr. Thomson.)

SIR,

It is well established that a degree of moisture is essential to the fermentation of vegetable matter, and also an elevation of temperature above the freezing point. About 60° of Fahrenheit's scale is considered favourable for many common processes in the arts; but I am not aware that it has been determined at what higher degree fermentation would be checked, and what still farther augmentation of temperature would entirely arrest its progress. In summer time good housewives frequently preserve milk from "turning sour," and also their conserves, by scalding. It is of importance to ascertain this augmentation of temperature exactly, particularly with regard to the operations for deoxidizing indigo. You will allow me, therefore, to make the inquiry. At the same time permit me to ask if it has been determined what degree of pressure has been found most economical and advantageous in the employment of steam, to work that grand but expensive instrument, the steam-engine?

Your obliged,

A. M.

C——, April 9.

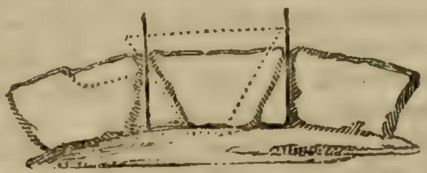
V. *On the Pavement of London.*

(To Dr. Thomson.)

DEAR SIR,

The bad state of the pavement in almost every street in London and Westminster where carriages of great burden are in the habit of going, cannot but be visible to every person who pays the slightest attention to the streets. It is a subject, I believe, which has lately engaged the particular attention of several public bodies, and may not be unworthy of a place in your *Annals*.

On passing the workmen now employed in paving that part of the Strand just beyond Somerset House, I observed that the stones were all shaped like wedges, having the narrower extremity downwards, viz. :—



I am not informed upon what principle this is done, but, according to my ideas, it does not appear to be the best method of securing the uniformity, or preserving the strength, of the pave-

ment. If the stones were laid alternately with the narrow and broad ends downwards, I am of opinion that a greater regularity would be preserved; for by drawing a line, as represented, down the intersertion between, it will be seen that as one of the stones is forced downwards, the adjoining ones are liberated in a proportionate degree.

It certainly would be a more effectual way to let the stones, if hewn in a wedgelike form, alternate with one another thus—

In this case a weight upon the centre stone would press equally upon the two adjoining ones; and a weight upon the stones having their broad ends downward would not be so liable to force them in as what it would upon a stone narrowed downwards.

I conceive, however, that as the stones are shaped for the purpose of paving, a different arrangement might be made of them, more conducive to strength and durability; for instance, an alternation of stones in the following shape,

laid together, would rest upon one another through the whole line, and, like an arch, would receive much strength and security by resting upon a strong curb or butment.

Should these observations be thought worthy of a place in your *Annals* for the next month, it will much oblige,

Dear Sir, yours very truly,

6, *Adam-street*, *April* 25, 1816. J. H. H. Holmes.

VI. *On the Ore of Copper described and analysed in the Annals of Philosophy, vol. vii. p.* 321.

(To Dr. Thomson.)

SIR,

From your late publication, I perceive you give us your own recent analysis of what you consider as a new ore of copper, in which you have considered the whole of the copper in a state of peroxide as combined with carbonic acid, and that the silex is a stranger to the constitution of the mineral.

I shall just remind you of the passage in the late tract of Berzelius, published by yourself, p. 35, and of the analysis of Lowitz published by Lucas, part 2d, p. 353, that afford reasonable ground for not rejecting the silex as a constituent in the mineral dioptase, which your new ore much resembles in its external characters.

That it ought not to be rejected in the analysis of your new ore, which, in my humble opinion, affords a strong proof of the truth of the electro-chemical theory, I trust you will not be displeased at receiving, or in my submitting to your serious consideration, the following application of your analysis to that theory.

6·1 gr. of silex = 25·31 gr. per cent. of silex = 37·87 gr. of oxide of silex.

10·5 gr. of copper = 43·57 gr. per cent. of copper = 52·22 gr. peroxide of copper.

Now adopting your experiment (the particulars of which are not stated) that the carbonate of copper is composed of one atom of acid and one atom of oxide; and if it shall appear probable that sufficient carbonic acid did not exist in the mineral to unite with the whole, but only half, the peroxide of copper, namely, 26·11 gr., the conclusion must be that the remainder of the copper is united to the silex.

26·11 gr. of peroxide of copper = 36·21 gr. of carbonate of copper.

Hence the ore is composed of

		contains oxyg.	
Oxide of silex	37·87	12·560	.. 3
Peroxide of copper united to silex	26·11	4·325	.. 1
Peroxide of copper united to carbonic acid	26·11	4·325	.. 1
Carbonic acid	10·11	4·257	.. 1
	100·20		

So that the contents of silex or copper are a little too great, owing probably to the whole of the oxygen not being liberated before weighing.

Hence the mineral is $Cu\,O^2 + Cu\,S^3$.

Berzelius's tables have been adhered to, which seem to differ from yours. I am, &c.

April 2, 1816. AN ELECTRO-CHEMICAL THEORIST.

I must have expressed myself inaccurately, otherwise this ingenious Correspondent would not have supposed that I was of opinion that the silica in the ore was only accidental; for I considered it as an essential ingredient, founding my opinion upon the different way in which nitric acid acts upon this ore, and upon common carbonate of copper. I did not venture to apply the atomic theory, because my analysis was made upon too small a quantity of the ore to warrant perfect confidence in the numerical results. But the statement in the preceding letter deserves attention, and there are strong reasons for considering it as accurate.

There is a considerable difference between the external characters of dioptase and of this mineral. The specific gravity of dioptase is

3·3, while that of our mineral is only 2·238. Dioptase is foliated, but our mineral is compact and conchoidal in its fracture. Dioptase is emerald-green, while our mineral is verdigris-green, with a tint of blue. Perhaps they may be only varieties or sub-species. But a greater number of specimens than I had an opportunity of seeing would be necessary to decide the point.—T.

VII. *On the Boiler of the Steam-Engine.*

(To Dr. Thomson.)

SIR, *Tradeston, Glasgow, April* 15, 1816.

From living in the neighbourhood of Mr. James Cook's extensive steam-engine manufactory, I have occasionally examined the new boilers and the old, which they have often to repair. I can easily see the propriety of the length and width of a steam-engine boiler, but I am at a loss to account for the depth. Does it not naturally suggest itself to you that two or three feet in depth, when the water presents the same surface, would serve the purpose as well as 20?

I have no doubt that Mr. Watt, when he first erected his engines, and before the boiler apparatus was so complete as he afterwards planned it, and is now in use, found it necessary to have them of a considerable depth, so as that the carelessness or neglect of the fireman for an hour or two might not injure its bottom. But now that the feed-pipe, or supply of water, is regularly wrought by the engine itself, and that supply from the hot cistern, it is certainly obvious that a considerable saving of expense in making the boiler, and a much greater in fuel, might be attained by lessening the depth.

I have likewise observed that almost the whole of the boilers which are repaired have been injured round the seat of the bottom. May not this be ascribed in a great measure to the turning of the plates, as the separating of the metal is visible in that operation, which admits the smallest particle of water to lodge, for water will sometimes escape from the most perfect made boiler, and run down its sides? Being so nigh the heat, the water is decomposed. Of course the metal becomes oxidated. This, I think, might be prevented, by bringing the almost separated particles of metal again in contact by hammering, as is practised in making the bottoms of copper boilers for use of brewers, &c. &c.

I have submitted the preceding hints to your observation that you may insert them in the *Annals of Philosophy*, if you think they are of sufficient consequence.

I am, Sir, your most obedient servant,

John Thomson.

I happened to be engaged to dinner with Mr. Watt on the day that I received the preceding letter; and as I knew that at first the boilers of steam-engines had been much shallower than they are now, I asked Mr. Watt the reasons which induced him to adopt the

present construction. He informed me that the sole object was to economise the fuel as much as possible. It is not the shallowness or depth of the boiler that produces this effect; but the making of the boiler of such a shape that the air which passes through the fire shall be robbed of almost all its heat before it can make its escape. Mr. Watt assures me that this object is very well attained by the present construction. I have sometimes thought that if the boilers were covered over with some bad conducting substances, they might perhaps require rather less fuel. But probably there is a good reason for the present practice, though I am not acquainted with it.—T.

VIII. *On Mr. Donovan's Essays, and the Mode of procuring pure Silver.*

(To Dr. Thomson.)

SIR,

I observe in the last number of the Philosophical Magazine that some reflections have been cast on the Royal Irish Academy for their reception of errors said to have been made by Mr. Donovan in one of his essays. He has apologised for that body, by answering that his opponent mistook the paper, and that they gave their prize to another of his productions. As I find in the same magazine that this will soon be published, and as the opinions which it contains are of a startling novelty, it is necessary to state that the Academy did not give Mr. D. *the* prize. In fact none of its members in the slightest degree admitted his conclusions; but in consideration of the labour which was apparent, he was allowed a portion of it to encourage his future efforts. I think it right that this should be known, as we are apt to judge of scientific bodies by their Transactions; at least I have often heard the Secretaries of the Royal Society arraigned for suffering particular articles to appear in the Transactions.

I see in the same publication a process proposed for purifying silver, by the same Gentleman, of which I request your opinion. It is generally thought that silver precipitated by copper always contains some of this metal, and I think it very unlikely that it can be removed by ammonia. Cautious crystallization will purify nitrate of silver considerably, as the nitrate of copper is much more soluble than the other; but Gay-Lussac has given a process which appears very plausible. He asserts that oxide of silver has a stronger affinity for nitric acid than that of copper. If, therefore, caustic potash be poured into the impure solution till a blue precipitate ceases to fall, it is evident that nothing but oxide of silver remains dissolved. As your extensive chemical inquiries must have made you familiar with the above subject, and others of a similar nature, it would be most acceptable to many of your readers if you would sometimes occupy a few pages of the *Annals* with an examination of the processes by which the different metals are obtained; more particularly as your System of Chemistry cannot, from its plan, be quite so diffuse as might be desired.

M. B.

Gay-Lussac's process succeeds tolerably well, but not unless a portion of the silver be precipitated. It is not, therefore, economical; though perhaps it might be so conducted that the loss of silver is not equivalent to the saving of nitric acid. Berthollet affirms that the silver precipitated by copper always contains some copper. But the quantity must be extremely small.. This is the process always, I believe, followed by manufacturers when they wish to recover silver held in solution by nitric acid. I consider the common method, by means of muriate of soda and pearlashes as good as any, if properly conducted. I have performed it I dare say a hundred times, and the loss upon an ounce of silver never exceeded a very few grains. The heat must not be too great nor too long continued, otherwise there is a risk of losing part, or even all, of the silver.—T.

IX. *Queries respecting Plano-cylindric Lenses.*

(To Dr. Thomson.)

SIR,

I observed some time ago, in one of the numbers of the Repertory of Arts, an account of a new mode of constructing lenses for dioptrical instruments, stated to possess very great and peculiar advantages over the spherical ones at present in use. A French artist, it seems, has taken out a patent for constructing optical instruments with those new lenses; from which I am led to conclude that the discovery (if it be really such) is of some importance. But on the other hand, I am led to adopt a different opinion, from the circumstance of your not having mentioned it in your publication.

However, as I am anxious to be informed on the subject, I beg you will take some notice of it in a future number of the *Annals.* I should be glad to know whether those cylindrical lenses recommended by the Frenchman were ever thought of, or tried, before; and if so, what are their peculiar properties; or if experiments have recently been made in this kingdom to ascertain their usefulness, what has been the result.

Again, as I have not been able to find any glass-grinder who would undertake to grind lenses of this new form (at least for a reasonable compensation), I should be greatly obliged to you, or some of your Correspondents acquainted with the art, to inform me of the best methods by which such lenses may be ground and polished; that, if possible, I might have some of them sufficient for such experiments as I might think proper to make, prepared under my own eye. This information I the more particularly request, as I do not find any account of the mode of grinding lenses in Imison, or in the Encyclopedia. As the form of the lenses, however, is new, a new mode must be resorted to for preparing them. If the artists in Edinburgh, Birmingham, or London, have done any thing in this way, it would be only necessary to refer to them. But residing at such a distance, I cannot easily make the necessary inquiry.

As the Repertory is extensively circulated, it is scarcely necessary to say that the lenses above-mentioned are equally cylindrical on both sides (whether convex or concave), and that the axes of the cylinders stand at right angles to each other. As these are said to produce no aberration of the rays of light, and give a correct representation of objects at every point of their surfaces, optical instruments may, by their means, possess a much greater degree of light than formerly, and a vastly larger field of view. Telescopes and microscopes of a given length may also bear eye-glasses of a much greater power than formerly.

Your mention of this subject, in whatever manner you may judge proper, will greatly oblige

Newry, April 3, 1816. A Constant Reader.

I noticed these glasses already in the *Annals of Philosophy*, vol. vii. p. 324. The trials made of them in this country did not promise much; but I believe all the lenses of the kind tried were too thick to give a fair idea of the value of the invention. It can hardly be expected that common spectacle grinders can make such lenses, because they are not provided with the necessary utensils. I am sorry that I cannot supply my Correspondent with the requisite information about grinding glasses, having never myself witnessed the methods, though they may be seen both here and in Birmingham, and probably in many other places.—T.

X. *Demonstration of a curious Relation between the various Orders of Differences.*

(To Dr. Thomson.)

Sir,

In the year 1772 the celebrated Lagrange announced in the Memoires de Berlin that a remarkable relation existed between the various orders of differences and their corresponding powers. Thus if e represent the number whose logarithm is unity, then

$$\Delta^n y = (e^{\frac{d y}{d x}} - 1)^n,$$

provided that n be any positive number, and that the exponents of the powers of $d\,y$ be transferred in the developement to the characteristic d.

The following demonstration of this important formula owes its origin to the perusal of an excellent paper by Dr. Brinkley on the same subject in the Philosophical Transactions for 1807. It is here offered as an inductive proof, and is perhaps better adapted to the comprehension of the young analyst than the one alluded to. The demonstrations given by Arbogast, Art. 399, Du Calcul des Derivations, and by Lacroix, Art. 864, Traité des Differences, are distinguished for their perspicuity and elegance.

Your obedient servant,

Plymouth, May 4, 1816. George Harvey.

Let $y_1, y_2, y_3, y_4,$ &c. represent the successive values of y; then we shall have by Taylor's theorem

$$\left.\begin{aligned}
y &= y \\
y_1 &= y + \frac{d\,y}{d\,x}\ \ w + \frac{d^2\,y}{d\,x^2} \cdot \frac{w^2}{1\,.\,2} + \text{\&c.} \\
y_2 &= y + \frac{d\,y}{d\,x}\,2\,w + \frac{d^2\,y}{d\,x^2} \cdot \frac{2^2\,w^2}{1\,.\,2} + \text{\&c.} \\
y_3 &= y + \frac{d\,y}{d\,x}\,3\,w + \frac{d^2\,y}{d\,x^2} \cdot \frac{3^2\,w^2}{1\,.\,2} + \text{\&c.} \\
&\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots \\
y_n &= y + \frac{d\,y}{d\,x}\,n\,w + \frac{d^2\,y}{d\,x^2} \cdot \frac{n^2\,w^2}{1\,.\,2} + \text{\&c.}
\end{aligned}\right\}\ldots\ldots(A)$$

But since by the theory of exponentials

$$e^x = 1 + x + \frac{x^2}{1\,.\,2} + \frac{x^3}{1\,.\,2\,.\,3} + \text{\&c.}$$

Therefore $e^x - 1 = x + \frac{x^2}{1\,.\,2} + \frac{x^3}{1\,.\,2\,.\,3} + \text{\&c.}$

and as x may be any function whatever, if we substitute for it successively

$\frac{d\,y}{d\,x}\,w,\ \frac{d\,y}{d\,x}\,2\,w,\ \frac{d\,y}{d\,x}\,3\,w,$ &c.

we shall obtain

$$\begin{aligned}
e^{\frac{d\,y}{d\,x}\,w} - 1 &= \frac{d\,y}{d\,x}\ \ w + \frac{d\,y^2}{d\,x^2} \cdot \frac{w^2}{1\,.\,2} + \text{\&c.} \\
e^{\frac{d\,y}{d\,x}\,2w} - 1 &= \frac{d\,y}{d\,x}\,2\,w + \frac{d\,y^2}{d\,x^2} \cdot \frac{2^2\,w^2}{1\,.\,2} + \text{\&c.} \\
e^{\frac{d\,y}{d\,x}\,3w} - 1 &= \frac{d\,y}{d\,x}\,3\,w + \frac{d\,y^2}{d\,x^2} \cdot \frac{3^2\,w^2}{1\,.\,2} + \text{\&c.} \\
&\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots \\
e^{\frac{d\,y}{d\,x}\,nw} - 1 &= \frac{d\,y}{d\,x}\,n\,w + \frac{d\,y^2}{d\,x^2} \cdot \frac{n^2\,w^2}{1\,.\,2} + \text{\&c.}
\end{aligned}$$

If now we transfer the exponents of the powers of $d\,y$ to the characteristic d, the second members of the preceding equations will become

$$\begin{aligned}
&\frac{d\,y}{d\,x}\ \ w + \frac{d^2\,y}{d\,x^2} \cdot \frac{w^2}{1\,.\,2} + \text{\&c.} \\
&\frac{d\,y}{d\,x}\,2\,w + \frac{d^2\,y}{d\,x^2} \cdot \frac{2^2\,w^2}{1\,.\,2} + \text{\&c.} \\
&\frac{d\,y}{d\,x}\,3\,w + \frac{d^2\,y}{d\,x^2} \cdot \frac{3^2\,w^2}{1\,.\,2} + \text{\&c.} \\
&\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots \\
&\frac{d\,y}{d\,x}\,n\,w + \frac{d^2\,y}{d\,x^2} \cdot \frac{n^2\,w^2}{1\,.\,2} + \text{\&c.}
\end{aligned}$$

And hence we may regard

$$e^{\frac{d\,y}{d\,x}\,w} - 1 = \frac{d\,y}{d\,x}\;w + \frac{d^2\,y}{d\,x^2} \cdot \frac{w^2}{1\,.\,2} + \text{\&c.}$$

$$e^{\frac{d\,y}{d\,x}\,2\,w} - 1 = \frac{d\,y}{d\,x}\;2\,w + \frac{d^2\,y}{d\,x^2} \cdot \frac{2^2\,w^2}{1\,.\,2} + \text{\&c.}$$

$$e^{\frac{d\,y}{d}\,3\,w} - 1 = \frac{d\,y}{d\,x}\;3\,w + \frac{d^2\,y}{d\,x^2} \cdot \frac{3^2\,w^2}{1\,.\,2} + \text{\&c.}$$

....

$$e^{\frac{d\,y}{d\,x}\,n\,w} - 1 = \frac{d\,y}{d\,x}\;n\,w + \frac{d^2\,y}{d\,x^2} \cdot \frac{n^2\,w^2}{1\,.\,2} + \text{\&c.}$$

provided in the developements of the first members we transfer the exponents of the powers of $d\,y$ to the characteristic d.

Hence we may obtain

$$y + e^{\frac{d\,y}{d\,x}\,w} - 1 = y + \frac{d\,y}{d\,x}\;w + \frac{d^2\,y}{d\,x^2} \cdot \frac{w^2}{1\,.\,2} + \text{\&c.}$$

$$y + e^{\frac{d\,y}{d\,x}\,2\,w} - 1 = y + \frac{d\,y}{d\,x}\;2\,w + \frac{d^2\,y}{d\,x^2} \cdot \frac{2^2\,w^2}{1\,.\,2} + \text{\&c.}$$

$$y + e^{\frac{d\,y}{d\,x}\,3\,w} - 1 = y + \frac{d\,y}{d\,x}\;3\,w + \frac{d^2\,y}{d\,x^2} \cdot \frac{3^2\,w^2}{1\,.\,2} + \text{\&c.}$$

....

$$y + e^{\frac{d\,y}{d\,x}\,n\,w} - 1 = y + \frac{d\,y}{d\,x}\;n\,w + \frac{d^2\,y}{d\,x^2} \cdot \frac{n^2\,w^2}{1\,.\,2} + \text{\&c.}$$

And since the second members of these equations are identical with the corresponding members of the equations represented by (A), we shall obtain

$$y = y$$

$$y_1 = y + e^{\frac{d\,y}{d\,x}\,w} - 1$$

$$y_2 = y + e^{\frac{d\,y}{d\,x}\,2\,w} - 1$$

$$y_3 = y + e^{\frac{d\,y}{d\,x}\,3\,w} - 1$$

....

$$y_n = y + e^{\frac{d\,y}{d\,x}\,n\,w} - 1$$

But by the theory of finite differences

$$\Delta\;y = y_1 - y$$

$$\Delta^2\;y = y_2 - 2\,y_1 + y$$

$$\Delta^3\;y = y_3 - 3\,y_2 + 3\,y_1 + y$$

....

$$\Delta^n\;y = y_n - n\,y_{n-1} + \frac{n\,(n-1)}{1\,.\,2}\,y_{n-2} - \text{\&c.}$$

And substituting the last found values of y_1, y_2, y_3 .... y_n, we shall obtain

$$\Delta\ y = \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots = e^{\frac{dy}{dx}w} - 1$$

$$\Delta^2\ y = e^{\frac{dy}{dx}2w} - 2\,e^{\frac{dy}{dx}\cdot w} + 1 \ldots\ldots\ldots = (e^{\frac{dy}{dx}w} - 1)^2$$

$$\Delta^3\ y = e^{\frac{dy}{dx}3w} - 3\,e^{\frac{dy}{dx}2w} + 3\,e^{\frac{dy}{dx}w} - 1 = (e^{\frac{dy}{dx}w} - 1)^3$$

$$\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$

$$\Delta^n\ y = e^{\frac{dy}{dx}nw} - n\,e^{\frac{dy}{dx}(n-1)w} + n\left(\frac{n-1}{2}\right)$$

$$e^{\frac{dy}{dx}(n-2)w} \pm 1 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots = (e^{\frac{dy}{dx}w} - 1)^n$$

which is the general formula given by Lagrange.

XI. *Sale of Minerals.*

We understand that the fine collection of minerals which belonged to the late Rev. Richard Hennah, of St. Austell, in Cornwall, will be sold by Mr. King early in June next.

XII. *New Arrangement of Primitive Rocks.*

Raumer, a German geologist of considerable eminence, has just published a small tract on the Granite of the Riesengebirge. The following is the succession of rocks which he describes as occurring in that country:—

1. *Central Granite.*—It is not intermixed with the adjacent rocks, and does not send out veins from its mass into the rocks that rest upon it.

2. *Gneiss and Granite Formation.*—This formation rests immediately on the central granite, and the gneiss and granite alternate with and pass into each other.

3. *Green-slate.*—This formation is principally composed of hornblende, and rests sometimes on No. 2, sometimes on No. 1.

4. *Gneiss.*—It rests upon the gneiss-and-granite formation, or the green-slate, and subordinate to it is a bed of mica-slate.

5. *Mica-slate.*—It contains great beds of lime-stone.

6. *Clay-slate.*

It deserves particular attention that all these five different formations are wrapped round the central granite in what is called a mantle-form position.

XIII. *Ordnance Maps of British Counties.*

The circumstances which were thought to render expedient the suspension of the publication of the Ordnance Maps being now removed, the publication of them is resumed, and they may be obtained, as formerly, at the Drawing-room in the Tower, or of Mr. Faden, Charing Cross. As the suspension was only intended to be temporary, not merely the operations of the trigonometrical survey, but those of the mapping and engraving, have been regularly carried on during that period, under the superintendance of

Col. Mudge; so that several county maps will be ready for delivery almost immediately. The maps of Cornwall, Devonshire, Dorsetshire, Hampshire (including the Isle of Wight), Sussex, and that part of Kent which squares in on the Sussex side, with the general work, will be published in a very few weeks: and a separate map of the Isle of Wight is now on sale. The maps of all the contiguous counties north of these are in the hands of the engravers: and that of the whole county of Kent is re-engraving, and in a state of forwardness. When the several plots and portions now planning by the surveyors are finished, at least three-fifths of England and Wales will be ready to be placed successively in the hands of the engravers; and the work will be carried on with all possible expedition consistent with accuracy. These maps are on a scale of an inch to a mile, a scale that admits of an attention to minutiæ which must, of necessity, be disregarded in maps of smaller size. Hence it may not only be expected that the general outline, and the prominent physical circumstances, shall be correctly delineated, but that the minuter points and peculiarities, which are interesting to the topographer and the antiquarian, shall be permanently marked, and readily traced, in these maps.

XIV. *New Hygrometer; and on the Mode of cutting Glass.* By Mr. Robert Burrhard.

(To Dr. Thomson.)

MY DEAR SIR, *Waddon House, March* 15, 1816.

As you were kind enough to notice an article I sent you a short time since, I am induced to trouble you again, by sending the sketch of a hygrometer on a new construction. As I have but just made the above, I cannot as yet state the results; but I think it will be very sensible and permanent; at least, it will answer as a comparative instrument to make others. Should you consider it worthy a place in your *Annals*, I shall feel myself honoured by your notice, and remain, dear Sir, very respectfully,

Rob. W. Burrhard.

The above-mentioned instrument (Plate L., Fig. *a*,) consists of the segment, A, B, C, made by joining together three very light pieces of flat brass, of which the pin, *e*, is the centre, on which hangs the long thin wire, D, to the lower end of which is attached a large piece of sponge, F. The arc, C, is to be divided decimally from 1 to 100. At *g* is a male thread, with a heavy nut, for the purpose of adjusting the pin, D, to 1; that is, to the beginning of the scale, or quite dry. At E is a common scale, beam, pin, and check, *f*, made with a fine bearing. If we suppose the sponge attached, and the instrument hung up, it must be adjusted by turning the heavy nut, *g*, either nearer or further from the point of bearing, *f*, until the line, D, with the sponge perfectly dry, comes exactly to the mark 1. The sponge must then be saturated with

moisture, which, from its increased weight, will make the arc, C, recede from 1 to 7 (but which should have been divided decimally); of course the intermediate numbers on the scale denote the degrees of dryness or moisture. From the greater part of this hygrometer being metal, it will not be liable to err, like the catgut, whipcord, &c.; and it may be adjusted at any time by merely drying the sponge, and noting whether it hangs directly at 1, which it must be made to do by turning the nut, *g*. The points of suspension, *e* and *f*, should be further apart than I have drawn them, as the instrument will be more sensible of change.

A more direct way of cutting a glass flask is, first to scratch it in the direction intended to be cut with a glazier's diamond, or common flint will answer; and then, by applying a red-hot poker to the end of the scratch, it may be carried in any direction with perfect ease.

Article X.

New Patents.

Samuel Jean Pauly, of Knightsbridge, Middlesex, engineer; for an article or substance for making without seams, coats, great coats, waistcoats, habits, cloaks, pantaloons, mantles, stockings, socks, and any other kind of clothing; covers for umbrellas, and hats, and mattrasses; seats and cushions filled with atmospheric air. March 23, 1816.

Samuel Brown, of Westgate, Norfolk, ironfounder; for improvements upon the swing and wheeled plough-carriage and plough-shares. March 23, 1816.

Robert Cameron, Jun., of Edinburgh, paper-maker; for a machine for manufacturing paper on a principle entirely new. March 23, 1816.

Emerson Dowson, of Welbeck-street, ironmonger, and John Isaac Hawkins, of Tichfield-street, engineer; for improvements or additions to grates and stoves, and an instrument, machine, or apparatus for supplying grates and stoves with fuel. March 23, 1816.

Uriah Hadock, of Holloway, chemist; for a new species of paint, colour, and cement, for painting and colouring and preserving the interior and exterior of houses, ships, and other things. March 23, 1816.

William Macnamara, of East Smithfield, plate-glass manufacturer; for a method or methods of manufacturing glass. March 23, 1816.

John Sorby, the younger, of Sheffield, edge-tool maker; for a method of making an augur, for the use of shipwrights, millwrights, carpenters, and other artificers, upon a new and improved construction. March 23, 1816.

Article XI.

METEOROLOGICAL TABLE.

1816.	Wind.	Barometer. Max.	Min.	Med.	Thermometer. Max.	Min.	Med.	Hygr. at 9 a. m.	Rain.	
4th Mo.										
April 19	S W	30·03	29·64	29·835	56	28	42·0	50		☾
20	S E	30·03	29·68	29·855	58	36	47·0	50		
21	N E	29·70	29·68	29·690	52	44	48·0			
22	S E	29·69	29·65	29·670	59	51	55·0	65	·32	
23	E	29·75	29·69	29·720	70	43	56·5	58		
24	N E	29·78	29·69	29·735	70	42	56·0	46		
25	E	29·99	29·78	29·885	68	39	53·5	61		
26	N E	29·99	29·94	29·965	68	39	53·5	56		
27	N E	29·94	29·81	29·875	68	37	52·5	56		●
28	S E	29·81	29·50	29·655	68	44	56·0	46		
29	W	29·50	29·44	29·470	67	47	57·0	57	—	
30	S E	29·54	29·42	29·480	61	36	48·5		5	
5th Mo.										
May 1	S E	29·66	29·54	29·600	62	42	57·0	41		
2	N W	29·87	29·66	29·765	63	34	48·5	61	—	
3	N W	29·93	29·83	29·380	59	48	53·5	60	·18	
4	S W	29·96	29·78	29·870	54	45	49·5	75	—	☽
5	N W	29·80	29·62	29·710	61	45	53·0	60	·22	
6	S W	29·86	29·80	29·830	58	40	49·0	46		
7					58	45	56·5		—	
8	S W	29·86	29·44	29·650	60	39	49·5	60	·50	
9	S W	29·52	29·40	29·460	56	39	47·5	63		
10					54	39	46·5		—	
11	N W	29·40	29·17	29·285	54	32	43·0	53	·55	○
12	N W	29·49	29·17	29·330	49	29	39·0	53	1	
13	W	29·75	29·49	29·620	57	30	43·5	55	1	
14	S E	29·77	29·75	29·760	62	37	49·5	74	5	
15	S W	29·78	29·77	29·775	64	44	54·0	54	1	
16	N E	29·78	29·64	29·710	72	49	60·5	48	1	
17	W	29·71	29·64	29·675	68	38	53·0			
18	N W	29·76	29·71	29·735	52	41	46·5	49		
		30·03	29·17	29·686	72	28	50·83	55	1·91	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Fourth Month. — 19. Cloudy, a.m.: cool dry wind. 20. Warm forenoon: about noon, a murmuring S. wind, with traces of a solar halo. 21. a. m. Obscurity above, with rudiments of the *Cumulus* beneath it: after this, thunder clouds in the S. horizon: rain followed these appearances, and continued during most of the forenoon: swallows appeared to-day: the hygrometer went to 70°. 22. Fine: *Cirrus*, *Cirrocumulus*, &c. 23. Very fine day; blue sky, with large *Cumuli*, and the lighter modifications above. 24. Warm forenoon: a smart easterly breeze, p. m.: the hygr. went to 35°: *Cirrus* predominated. 25. Brisk wind at N. E. and S. E: the sky clear and pale. 26. Fine day: steady breeze. 27. Much dew: clear morning: then *Cumulostratus*, with a breeze. 28. Dew: clear morning: *Cirrostratus* appeared, passing afterwards to *Cumulostratus*: at sun-set, *Cirrus* appeared above. 29. Little or no dew: the sky full of a confused mixture of *Cirrus*, *Cirrocumulus*, &c.: some drops of rain, followed by more in the night. 30. Overcast: dripping.

Fifth Month.—1. Fair. 2. Cloudy at intervals, with a few drops: much *Cirrostratus* to the westward. 3. Rain at intervals, chiefly in the night. 4. Completely overcast, a. m. with *Cirrostratus*: a wet day. 8. Very rainy, p. m. after a little hail about noon. 9. A little rain, a. m.: some sunshine, p. m. 10. Rainy the whole day. 11. Fair in the evening. 13. A little rain, p. m. 17. Very fine day: cool evening. 18. Fair, but cold.

RESULTS.

Prevailing Winds Easterly in the fore part, and Westerly, with rain, in the latter part, of the period.

Barometer:	Greatest height	30·03 inches.
	Least	29·17
	Mean of the period	29·686
Thermometer:	Greatest height	72°
	Least	28
	Mean of the period	50·83

Mean of the Hygrometer .... 55°

Rain........................... 1·91 inch.

During this period the leafing of the more forward trees has proceeded, for the most part, under the retarding influence of cold breezes. Twice, the temperature having risen for a few days, the accumulation appears to have gone off in local thunder-storms. In travelling on the 17th inst. from Bristol to Southampton, I had the rare opportunity of observing, from a convenient distance, the gradual formation and discharge of a prodigious *Nimbus*, forming part of a series of clouds which for several hours continued to pour a flood of rain, accompanied by large hail, thunder, and lightning, on the country about Andover and Winchester. As the sun, which was declining, strongly illuminated these clouds, they reflected a lively copper tint above the indigo ground which marked the heavy rain: the electrical light which fills the striking cloud at each discharge was, therefore, with the stroke itself, imperceptible; as was the thunder, from the distance: and the phenomena I had to supply (as to certain evidence) from subsequent information as we passed over the tract thus plentifully irrigated.

Tottenham, *Fifth Month*, 24, 1816. L. HOWARD.

INDEX.

C.

D.

I.

K.

L.

M.

N.

O.

P.

ERRATA IN VOL. VI.

P. 454, line 17, *dele* that.
— 470, — 18, *for* and alcohol and oil, *read* and ether and oil.
— 470, — 21, — burn and, *read* burnt.

ERRATA IN VOL. VII.

P. 192, line 24, *for* Padrift, *read* Tadrift.
— 194, — 29, — stellage, *read* kenbledge.
— 206, — 31, — D T, *read* D F.
— 226, — 5, — dilated, *read* diluted.
— 306, — 17, — 123·37, *read* 122·37.
— 321, — 49, — and is composed, *read* is composed.
— 386, — 9, — Tigarga, *read* Tigarea.
— 397, — 35, — Thenard, *read* Proust.
In Plate XLV. Fig. 16, should be a right-angled triangle.
In Plate XLIX. *for* Sulitebina, *read* Sulitelma.

END OF VOL. VII.

C. Baldwin, Printer,
New Bridge Street, London.